

## POSITIVISM AND ITS CRITICS

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“POSITIVISM” has today become more of a term of abuse than a technical term of philosophy. The indiscriminate way in which the term has been used in a variety of different polemical interchanges in the past few years, however, makes all the more urgent a study of the influence of positivistic philosophies in the social sciences.

I shall distinguish two main ways in which “positivism” may be taken, one quite specific, the other much more general. In the more restrictive sense, the term may be taken to apply to the writings of those who have actively called themselves positivists or at least have been prepared to accept the appellation. This yields two major phases in the development of positivism, one centered mainly in social theory, the other concerned more specifically with epistemology. The earlier phase is that dominated by the works of the author who coined the term “positive philosophy,” Auguste Comte. Although there are obvious contrasts between Comte’s positivism and the “logical positivism” of the Vienna Circle, there are equally clear connections—both historical and intellectual—between the two. However, the term may also be employed more broadly and diffusely to refer to the writings of philosophers who have adopted most or all of a series of connected perspectives: phenomenalism—the thesis, which can be expressed in various ways, that “reality” consists of sense impressions; an aversion to metaphysics, the latter being condemned as sophistry or illusion; the representation of philosophy as a method of analysis, clearly separable from, yet at the same time parasitic upon, the findings of science; the duality of fact and value—the thesis that empirical knowledge is logically discrepant from the pursuit of moral aims or the implementation of ethical standards; and the notion of the “unity of science”—the idea that the natural and social sciences share a common logical and perhaps even methodological foundation.

In this chapter, I shall use the term “positivism” without qualification to refer, in the appropriate context, to the views of Comte and subsequently to

those of the leading figures of the Vienna Circle—in other words, to those who have been prepared to call themselves positivists. I shall use “positivistic philosophy” to designate views that embody important elements among those mentioned in the second category. In this sense, positivistic strains are much more widely represented in the history of philosophy, overlapping with empiricism, than would be suggested if attention were confined to self-proclaimed “positivism.”

However, I also want to distinguish a third category, which I shall call, for want of a better name, “positivistic sociology.” We owe to Comte both the term “positivism” and the term “sociology”; in his writings, the two are closely conjoined, since the coming into being of sociology is supposed to mark the final triumph of positivism in human thought. The connection has been a fateful one for the subsequent development of the social sciences, for certain leading traditions in social thought over the past hundred years have been considerably influenced by the kind of logical framework established by Comte in his *Cours de philosophie positive*. As mediated by Durkheim, this framework is closely tied in to modern functionalism. But the influence of positivistic philosophy as defined above in sociology (and in Marxism) has ranged much more widely than this. Here sociology is conceived of as a “natural science of society” which can hope to reproduce a system of laws directly similar in form to those achieved in the natural sciences. In positivistic sociologies, at least, as formulated over the past four or five decades especially in the United States, all three senses of “positivism” I have just distinguished to some extent recombine. Several of the prominent members of the Vienna Circle emigrated to the United States and have exerted a strong influence over the development of philosophy there, particularly with regard to the philosophy of science. Their conception of the philosophy of science has in turn been appropriated, explicitly or otherwise, by many authors writing in the social sciences, and it has proved particularly compatible with the ideas of those drawing heavily upon the sorts of views expressed by Comte and by Durkheim.

In this chapter, I shall begin by discussing the positivism of Comte and its similarities to and differences from the logical positivism of the Vienna Circle. From there, I shall move to a consideration of two partly convergent critiques of positivistic philosophies more generally conceived: one, the so-called “newer philosophy of science,” emanating mainly from within the English-speaking world; the other, “Frankfurt philosophy” or critical theory, originating primarily in long-established German philosophical traditions.

### Auguste Comte: Sociology and Positivism

In crude summary, we may differentiate several major elements in the intellectual background of Comte's writings. One is the frontal assault on metaphysics undertaken in eighteenth-century philosophy, above all in the

works of Hume and his followers in British empiricism, and sustained in different form in Kant's “critical idealism.” Comte went further than such authors, not only in accepting the success of the destruction of transcendental illusions, but in formally embodying the metaphysical stage in the evolution of humanity as a phase superseded by the advent of positivist thought. In this respect, he accepted one of the fundamental aims of the writers of the Enlightenment, as he did important aspects of the rationalist critique of established religion. In Comte's scheme of history, the theological stage of thought is relegated to a phase prior to the metaphysical—both, to be sure, regarded as necessary stages in social evolution, but both being dissolved once and for all when positivism triumphs. If Comte himself came to the rediscovery of religion, it was because he combined these aspects of Enlightenment philosophy with a deep-rooted aversion to the methodical critique of inherited authority that was basic to the writings of the *philosophes*. Comte rejected the essential idea of “Enlightenment” itself: that the Middle Ages were also the Dark Ages, whose repudiation opens up the way to revolutionary changes in human intellectual and social life. In place of this Comte substituted a progressivism influenced by the “retrograde school” of authors—conservative apologists for Catholicism, reacting against Enlightenment radicalism and against the 1789 revolution which was its heir—such as Bonald, de Maistre, and others. Comte's positivism preserves the theme of progress but undercuts the radicalism with which this was associated in Enlightenment philosophy. “Progress” and “order” are more than reconciled: the one becomes dependent upon the other. Positive thought replaces the “negative” outlook of the *philosophes*, the perspective that a new dawn can be achieved through the shattering of the past.

Of course, Comte owed many of his ideas most immediately to Saint-Simon, who in turn was considerably indebted to Condorcet and Montesquieu, who both had tempered the enthusiasms of the Enlightenment with a rigidly applied version of the subservience of society to natural laws of development. Condorcet assigned to history the same kind of potentialities that Comte was later to allocate to the positive science of sociology, expressed in the famous phrase “*savoir pour prévoir, prévoir pour pouvoir*.” Condorcet looked to the past to supply the moving principles of evolution whereby the future could be made open to human intervention. Hence he took to task those who arrogantly supposed that it is possible to achieve social change in massive fashion *ex nihilo*. The progress of mankind achieves equilibrium in such a way that, while the pace of development can be speeded or retarded by active human intervention, it has the character of an autonomous force for betterment. I shall not take up the vexed issue of just how directly Comte plundered Saint-Simon's ideas in constructing his own system, a matter of great acrimony in the relations between the two thinkers after Comte broke away from the tutelage of his mentor. Whatever their immediate provenance, it can be remarked without undue simplification that Comte's writings constitute one direction of development out of Saint-Simon, that which gave “sociology” its name and established a logical framework for the supposedly

new science. The other direction is that taken by Marx, in which elements of Saint-Simon's ideas are reconnected to revolutionary social transformation.<sup>1</sup>

That Comte entitled the first of his two major works *Cours de philosophie positive* should not blind us to the fact that the work actually declares an end to philosophy as previously practiced: as an independent enterprise separable from the achievements of science. "Positive philosophy" is perhaps not, as Marcuse suggests, a contradiction *in adjecto*.<sup>2</sup> But it does reduce philosophy to expressing the emergent synthesis of scientific knowledge. The "true philosophic spirit," Comte says, incorporates the "essential attributes . . . summed up in the word *positive*." These include, first of all, an orientation to "reality" and to "utility": the useless endeavors of speculative philosophy to penetrate behind appearances are disavowed. But the term also implies—in all the European languages, according to Comte—"certainty" and "precision," attributes which similarly distinguish the intellectual life of modern man from his predecessors. Finally, also suggested by the term are an "organic tendency" and a "relativist outlook." The former of these refers to the constructive character of the positivist spirit: by contrast, "the metaphysical spirit is incapable of organizing; it can only criticize." The latter seals the rejection of absolutism as characteristic of metaphysical philosophy: the laws that govern the covariance of phenomena always retain a provisional character, since they are induced on the basis of empirical observation, rather than being posited as "absolute essences."<sup>3</sup>

In the *Cours*, the relation between the various sciences is claimed to be hierarchical, in both an analytical and a historical sense, the second being explained in terms of the renowned law of the three stages of human intellectual development. Analytically, Comte makes clear, the sciences form a hierarchy of decreasing generality but increasing complexity; each particular science logically depends upon the ones below it in the hierarchy and yet at the same time deals with an emergent order of properties that cannot be reduced to those with which the other sciences are concerned. Thus biology, for example, presupposes the laws of physics and chemistry insofar as all organisms are physical entities which obey the laws governing the composition of matter; on the other hand, the behavior of organisms as complex beings cannot be directly derived from those laws. Sociology, at the apex of the hierarchy of sciences, logically presupposes the laws of each of the other scientific disciplines while similarly retaining its autonomous subject matter.

The logical relations between the sciences, according to Comte, provide the means of interpreting their successive formation as separate fields of study in the course of the evolution of human thought. The sciences which developed first—mathematics and astronomy, then physics—were those dealing with the most general or all-enveloping laws in nature that govern phenomena most removed from human involvement and manipulation. From there, science penetrated more and more closely to man himself, moving through chemistry and biology to its culmination in the science of human conduct—originally labeled "social physics" by Comte, then redubbed "sociology." The process is not achieved without struggle; scientific understanding lies at the

end of the progression of intellectual life through the theological and metaphysical stages. Human thought as a whole, as well as each science taken separately, progress through the theological, the metaphysical, and the positive stages. In the theological stage, the universe is comprehended as determined by the agency of spiritual beings; this stage—*l'état fictif*, as Comte calls it—is "the necessary point of departure of the human intellect," and it reaches its climax in Christianity with its recognition of one all-powerful deity.<sup>4</sup> The metaphysical phase replaces these moving spirits with abstract essences, thereby clearing the ground for the advent of science, *l'état fixe et définitif* of thought. The enunciation of the law of the three stages, Comte says, is enough "that its correctness should be immediately confirmed by anyone who has a sufficiently profound knowledge of the general history of the sciences." (Comte later claimed to have achieved personal verification of the law of the three stages in his periods of insanity, which he had experienced, he claimed, as a regression back through from positivism to metaphysics to theology on the level of his own personality, in his recovery retracing these stages forward again.)

The task of the *Cours* is not only to analyze the transmutation of human thought by science, but also to *complete* it. For man's understanding of himself is still in substantial part in its prescientific phase:

Everything can be reduced to a simple question of fact: does positive philosophy, which over the two past centuries has gradually become so widespread, today embrace all orders of phenomena? It is evident that such is not the case and that consequently there still remains the major scientific undertaking of giving to positive philosophy the universal character that is indispensable to its proper constitution. . . . Now that the human mind has founded astronomy, and terrestrial physics—both mechanical and chemical—and organic physics—both botanical and biological—it remains to finalise the system of the sciences by founding *social physics*. Such is, in several capital respects, the greatest and the most pressing intellectual need today. . . .<sup>5</sup>

Positivism supplies a general ground plan for the formation of sociology; that is to say, the new science of society has to share the same overall logical form as the other sciences as it is cut free of the residues of metaphysics. But since the phenomena with which it is concerned are more complex and specific than the sciences lying below it in the hierarchy, it also has to develop methodological procedures of its own. Like biology, sociology employs concepts that are "synthetic" in character; that is to say, concepts which relate to the properties of complex wholes rather than to aggregates of elements as in the lower sciences. The two also share a division into statics and dynamics. In sociology, the first consists in the study of the functional interrelationship of institutions within society, the second in the study of the process of social evolution. The significance of dynamics in sociology, however, is more profound than in biology, because—via the law of the three stages—it examines the intellectual development of positive thought as a whole. Sociology relies on three methodological elements, each of which involves features that are particular to it: observation, experiment, and comparison. Comte holds that a commitment to the essential importance of

empirical observation is not equivalent to an advocacy of empiricism. "No logical dogma," Comte says, "could be more thoroughly irreconcilable with the spirit of positive philosophy, or with its special character in regard to the study of social phenomena, than this."<sup>6</sup> Consequently, theory is basic to sociological investigations. On the other hand, the context of Comte's discussion makes it apparent that "empiricism" here is understood in a limited sense; his point is not that all observations of objects or events are (to use Popper's term) "theory impregnated," but that "scientifically speaking, all isolated, empirical observation is idle." "Scientific and popular observation," Comte says, "embrace the same facts"; but they regard them from different points of view, because the former is guided by theory whereas the latter is not. Theories direct our attention toward certain facts rather than others.<sup>7</sup> While experimentation in the laboratory sense is not possible in social physics, it can be replaced by indirect experimentation—that is, "natural experiments" whose consequences can be analyzed. But this is less important than the comparative method, which is the crucial foundation of sociological research.

Comte always intended sociology to be directed toward practical ends. If it is true that the strange extravagances of the immanent social future envisaged in the *Système de politique positive* are largely absent from Comte's earlier writings, it is still the case that the main elements of his political program already appear there. These are perhaps stated with greater clarity, in fact, in the *Cours* than they are in the later work. The overriding theme continues that of the intellectual diagnosis of the origins of positive philosophy: the mutual necessity of order and progress. For Comte it is precisely his insistence upon the conjunction of the two that allows positivism to supersede both the "revolutionary metaphysics" of the *philosophes* and the reactionary connotations of the Catholic apologists. The latter school wanted order but was against progress; the former sought progress at the expense of order. The "order" desired by the "retrograde school" was nothing but a reversion to feudal hierocracy; while the "progress" aspired to by the revolutionaries was nothing less than the subversion of any form of government as such. The sort of society Comte foresees as guaranteeing order and progress nonetheless places a heavy enough emphasis upon features that brook large in the writings of the members of the "retrograde school"—moral consensus, authority, and an antagonism to the "chimera of equality"—even if stripped of their specific association with Catholicism. At first sight the call to establish a Religion of Humanity seems quite inconsistent with the positive philosophy advocated in the *Cours*, and many commentators have supposed that there is a major hiatus between Comte's earlier and later works.<sup>8</sup> But it is perhaps more plausible to argue that the *Système de politique positive* brings fully into the open the latent substratum of the positive spirit: we see that science cannot, after all, provide its own commitment.

How, even so, can a perspective which insists that the course of human social development is governed by laws akin to laws of nature provide any

leverage for rational human intervention in history? Doesn't this imply the adoption of fatalism in the face of the inevitable sweep of social change? According to Comte, the contrary is actually the case. For the rational facilitation of progress is only possible if the limiting conditions of intervention are known; the laws that control the movement of society are subject to considerable margins of variation in their operation, and such variation can be actively influenced by deliberate action.<sup>9</sup>

### Comte's Influence: The Origins of Logical Positivism

Although his writings had rather little immediate influence in France, Comte's works attracted a considerable following abroad: in other European countries, the United States, and particularly Latin America. In Britain, the *Cours* acquired a notable admirer in John Stuart Mill, and Mill's *Logic* was in important respects its counterpart in English-speaking social thought. Many such followers were alienated, however, by the drift of Comte's thought in the later part of his career, as expressed in the *Système de politique positive*, which Mill called "this melancholy decadence of a great intellect." As a social movement, which Comte had all along tried to make it, positivism died with the withering of the groups of disciples who remained to celebrate the Festival of Humanity held in London in 1881. I shall not be interested here in trying to detail in what ways Comte's works were drawn upon by other authors, during his lifetime or after it: some prominent contemporaries, most notably Herbert Spencer, were anxious to claim a greater independence between their ideas and those of Comte than seems in fact to have been the case.<sup>10</sup> I shall consider the influence of Comte only from two aspects: the mode in which his writings were utilized by Durkheim and the extent to which Comte's views conform intellectually to the philosophical program developed in logical positivism.

The importance of the line of connection from Comte to Durkheim is easily attested. So far as social science in the twentieth century is concerned, the influence of Comte's writings derives less from their direct impact than from their reworking in Durkheim's version of sociological method. Durkheim's works have provided the proximate source of functionalism in both anthropology and sociology. But Durkheim's work has also had a more broad ranging and diffuse effect as a stimulus to those central traditions of contemporary social thought in which the goal of achieving a "natural science of society" is considered both desirable and feasible.<sup>11</sup>

In Durkheim, the methodological framework of Comte's positivism, which is sustained, is separated from the global theory of historical change, which is largely abandoned. Durkheim makes this quite explicit. Comte regarded

Condorcet and Montesquieu as forerunners who established the groundwork of the positivist spirit but nonetheless were unable to detach themselves adequately from the speculative philosophy of history. Durkheim has much the same view of the two former thinkers, but lumps Comte along with them as belonging to the prescientific phase in the history of sociology. The "law of the three stages," according to Durkheim, is proclaimed by fiat rather than corroborated empirically: a massive research undertaking, well beyond the capacity of any single scholar, would be required to document adequately such a principle of social change.<sup>12</sup> In this respect, Durkheim's comments concur with the judgement of Mill: "M. Comte, at bottom, was not so solicitous about completeness of proof as becomes a positive philosopher."<sup>13</sup>

Durkheim's discussions of social evolution and his diagnosis of the trend of development of modern industrial civilization owe as much to Saint-Simon and to the German "academic socialists" as they do to Comte. But the influence of Boutroux and others notwithstanding, it is undeniably the legacy of Comte that looms largest in the methodological scheme of sociology which Durkheim set out. While Durkheim does not endorse the "hierarchy of the sciences" as such, he insists perhaps even more strongly than Comte upon the autonomy of sociology as a distinctive field of endeavor. Like Comte, he holds that recognition of such autonomy does not imply that the study of human social conduct is logically discrepant from natural science; social facts have a moral dimension that is absent in nature, but they have "to be treated as things" in the same manner as natural objects. The aim of sociology is to arrive at the formulation of principles that have the same objective status as natural scientific laws. In Durkheim, a Baconian version of scientific method is perhaps more apparent than in Comte. Every science, Durkheim says, including sociology, advances only slowly and cautiously, through patient inductive generalization based on observed regularities in social facts. This is, indeed, why he is critical of Comte's claims to have established a positivist account of history. When Durkheim refuses the designation "positivist" in favor of "naturalism," he seeks to dissociate his general position from that of Comte while reaffirming the character of sociology as a natural science of society. Durkheim's account of the emergence of the scientific spirit, although not elaborated in anything like the historical detail attempted by Comte, actually follows the outline of Comte's discussion very closely. All thought originates in religion, Durkheim holds, (and he tries to explain how this should be the case in *The Elementary Forms of Religious Life*); it can be demonstrated that even the Kantian categories are first of all religious concepts.<sup>14</sup> The key differences between prescientific and scientific thought are methodological; "thought and reflection are prior to science, which merely uses them methodologically."<sup>15</sup> As religious concepts become secularized in the form of metaphysical philosophy, they become more precise, but they are finally rendered scientific only by being anchored in empirical observation and thereby transformed.

It is clear that Durkheim derives his conception of functionalist method from Comte and not from Spencer. Durkheim follows Comte closely in

separating out functional explanation (statics) from historical explanation (dynamics), although he criticizes Comte along with Spencer for reifying "progress": treating the impetus to self-betterment as if it were a general cause of the evolution of society. As in Comte's writings, and of course in those of many other nineteenth-century writers also, Durkheim's stress upon the significance of functional explanation in sociology comes fairly directly from the model of biology, as does his acceptance of "holistic" concepts as basic to sociological analysis. However, the biological parallel also provides another very important element in Durkheim's works, bearing immediately upon the practical implications of social science. In claiming that the scientific study of society can provide the means of distinguishing what is normal from what is pathological in any particular type of society, Durkheim upholds the most intrinsic part of Comte's program for positivism. For just as natural science shows us that the development of knowledge can only be achieved incrementally, so sociology shows us that all truly progressive social change occurs only cumulatively. The mutual dependence of progress and order is as much a theme of Durkheim's writings as it is of Comte's writings. Durkheim's antagonism to revolution continues that of Comte and is likewise held to be grounded scientifically: political revolution expresses the inability of a society to generate progressive change, rather than itself providing a possible instrument of securing social transformation. However, while the form of the account is similar, the content is not wholly the same; that is to say, in identifying what is normal and what is pathological in contemporary society, and thus specifying the immanent trend of social development, Durkheim moves away substantially from Comte.<sup>16</sup>

In mentioning these respects in which Durkheim was indebted to Comte, I do not, of course, mean to claim that Durkheim's works can be regarded as little more than an extension of those of the earlier thinker. But I do hold that Durkheim's writings have been more influential than those of any other author in academic social science in the spread of "positivistic sociology" as I have defined that term. Through them, Comte's "positivism" has had a major influence upon the more diffuse development of such positivistic sociology. This is one line of filiation leading from Comte through to twentieth-century thought. The other is less direct, connecting Comte to the logical positivism of the Vienna Circle.

The principal mediator between Comte's positivism and the positivism of the Vienna Circle is normally held to be Ernst Mach, the physicist and physiologist. Mach, like Durkheim, rejected the label "positivist" and, unlike Durkheim, was not directly influenced by Comte save in minor respects.<sup>17</sup> The importance of Comte in relation to Mach is really in helping to further the intellectual currents that were in the background of Mach's work as a natural scientist. The following elements in Comte's thought are relevant in this respect:

1. The reconstruction of history as the realization of the positive spirit. In this scheme of things, religion and metaphysics have a definite place, but only as prior phases of mystification to be broken through by the advent of science.

With the development of the scientific outlook, the "prehistory" of the human species is completed; the positive stage of thought is not a transitional one like the other.

2. The final dissolution of metaphysics, closely linked to the idea of the supersession of philosophy itself. In Comte's positivism, science replaces philosophy: "positive philosophy" is the logical explication of the canons of scientific method. Metaphysics is not accorded the status of being open to philosophical discussion in its own right: it is consigned to the lumber room of history on the basis that the questions posed in metaphysical philosophy are empty of content.

3. The existence of a clear and definable boundary between the factual, or the "observable," and the imaginary, or the "fictitious." Comte does not provide an ontological justification of what counts as factual, but rather a methodological one.<sup>18</sup> It is in this regard, his disavowals notwithstanding, that Comte adopts the standpoint of empiricism. Systematic observation supposedly distinguishes positive science from other claims to knowledge, and such observation, according to Comte, depends upon the evidence of sense perception; this is the ground of certainty in science. The rationalist features of Comte's thought do not enter in at this level, but only at the level of the selective organization of facts within theories: theories provide for the *connection* of facts to universal propositions or laws.

4. The "relativism" of scientific knowledge. "Relativism" here is not used in the sense which it has subsequently come to acquire: the acceptance, in some form or other, of multiple worlds of reality. That is to say, it is again not an ontological term but refers to the thesis that science confines itself to explaining the interdependence of phenomena: it does not claim to discover essences or final causes. Scientific knowledge is never "finished" but is constantly open to modification and improvement.

5. The integral tie between science and the moral and material progress of mankind. Comte's adoption of the Baconian formula that the foreknowledge yielded by science makes possible technological control, the integration of "prevoir" and "pouvoir," expresses this exactly. This not only unifies science and technology but extends the realm of the technological to human social development itself; as Comte says quite explicitly, technology will no longer be exclusively associated with the physical but will become "political and moral."<sup>19</sup>

Each of these views reappears in Mach's writings, although not of course in identical form to their expression in Comte's works. There is nothing in Mach comparable to Comte's massive endeavor to synthesize scientific knowledge within a scheme that is simultaneously historical and analytical. But Mach was directly influenced by theories of evolution and saw in Darwin and Lamarck a basis for explaining the emergence of scientific thought from the entanglements of metaphysics. For Mach, the scientific outlook triumphs historically and finds its moral justification in facilitating the survival and welfare of the human species.<sup>20</sup> Mach uses the term "philosophy" with the same dual connotation as Comte. When he writes that he is not a philosopher

and that science does not rely on any particular type or system of philosophy, Mach echoes Comte's theme of the abolition of philosophy. "Philosophy" here is used to mean transcendent or "metaphysical philosophy"; both Comte and Mach proclaim an end to philosophy in this sense. Where Comte and Mach speak of the retention of philosophy, on the other hand, it is as *philosophie positive*: philosophy here is the logical clarification of the bases of science. "There is above all no Machian philosophy," Mach emphasizes. There is at the most "a natural-scientific methodology and a psychology of knowledge," and these latter "are like all scientific theories provisional, incomplete attempts."<sup>21</sup> Mach's dismissal of metaphysics is as complete as that of Comte, although linked to a more thorough-going phenomenalism that Comte ever adopted:

I should like the scientists to realize that my view eliminates all metaphysical questions indifferently, whether they be only regarded as insoluble at the present moment, or whether they be regarded as meaningless for all time. I should like then, further, to reflect that everything that we can know about the world is necessarily expressed in the sensations, which can be set free from the individual influence of the observer in a precisely definable manner. . . . Everything that we can want to know is given by the solution of a problem in mathematical form, by the ascertainment of the functional dependency of the sensational elements on one another. This knowledge exhausts the knowledge of "reality."<sup>22</sup>

For Mach, scientific knowledge is "relative" in Comte's sense; the object of science is to discover relations between phenomena. According to Mach, however, this carries the implication that theory has a purely heuristic role in scientific investigations. The precise identification of the mathematical functions that express the dependencies between phenomena in nature renders theory obsolete. In Mach's phrase, theories resemble dead leaves which fall away when the tree of science no longer has a need to breathe through them. Although this is distinct from Comte's view, it is not as far removed from it as may seem the case at first blush. In his discussion of the positive method of science, Comte commingles empiricism and rationalism: as I have already mentioned, however, he does so by treating theory as the mode of organizing fact in a way relevant to scientific procedure.

In Comte's positivism, no place is found for the reflexive subject; psychology does not even appear in the hierarchy of the sciences, and the notion of subjective experience is regarded as a metaphysical fiction. In this regard, Comte stands in direct line of descent from Hume. But this is a standpoint that is taken for granted in Comte's writings rather than defended in detail. Mach, however, confronts the issue directly, and his stand upon it is quite unequivocal. The self or ego does not exist as a unity; it is merely an aggregate of sensations. According to Mach, if this is accepted, it disposes of the accusation of solipsism that is frequently made against phenomenalism; since the self does not exist, there can be no question of the isolation of the self in the universe. Mach saw no discrepancy between this view and either the existence of morality or the role of science in furthering the betterment of humanity. It is antireligious insofar as it has the consequence that there can



be no survival of the soul after death, since "I" has no unitary existence anyway—although in the latter part of his career Mach came to see affinities between his standpoint and the world view of Buddhism. Mach believed that his view, far from rejecting the ethical value of the individual personality, enhances it by preventing an overevaluation of the "self"; it places the emphasis on the moral welfare of mankind as a whole. This links back to Mach's conception of the relation between science and human progress: the triumph of the scientific spirit provides both a technological and a moral basis for the evolutionary advancement of man.

Mach's writings and teachings both helped to foster a climate of opinion in Vienna propitious for the development of what came to be known as logical positivism or logical empiricism (the latter being the term preferred by Schlick) and also directly influenced ideas of the most prominent members of the Vienna Circle.<sup>23</sup> But the logical positivists drew heavily upon other sources also, and in certain respects their work contrasts quite clearly with that of Mach. Beginning with the group formed in 1907 around Frank, a physicist, Neurath, an economist, and Hahn, a mathematician, the logical positivists sought to develop a view of science which would recognize the vital significance of logic and mathematics in scientific thought as systems of symbolic representations. This led them to acknowledge the central importance of language: a theme which connects their writings to the major thrust of development of philosophy as a whole in the twentieth century. One line of thought leading in this direction within the philosophy of science was that provided by Poincaré's conventionalism, sometimes referred to as the "new positivism." Schlick and others were critical of conventionalism but recognized the force of the claim that scientific theories embody linguistic conventions. The thesis that theories are languages for the representation of facts, stripped of some of the skeptical features of conventionalism, was taken over as a key element of logical positivism.

But in their approach to the mode of analyzing the content of such languages, the logical positivists were indebted to British philosophy. What has been called the "revolution in British philosophy,"<sup>24</sup> led by Moore and Russell, was initiated by them as a reaction to the Hegelianism of Bradley, McTaggart, and others. It was both a return to the traditions of British empiricism and a new departure. Russell himself did not set out to discredit metaphysics; rather, he believed that philosophy should become rigorous and precise and that the way to achieve this goal lay through the logical elucidation of the language in which scientific theories are couched. Philosophy is to reveal the logical structure which underlies the superficial play of appearances. Russell's object was not, like that of Husserl's transcendental reduction, eventually to recover the everyday world of common sense or of the "natural attitude," but to provide an account that would conform to established scientific knowledge. Russell's "logical atomism" had a strong influence on the young Wittgenstein, and it was partly through Wittgenstein's personal contacts with some of the Vienna Circle, and through his *Tractatus*, that these ideas were communicated. Wittgenstein's impact upon

the members of the Vienna Circle has been so frequently emphasized, however, that it is worthwhile pointing out that Carnap, ultimately the most influential of the group, has acknowledged Frege and Russell as having had the strongest effect upon his philosophical development. He attended Frege's lectures in Jena and through them was introduced to the *Principia Mathematica*; Hahn had independently acquainted the members of the Vienna Circle with the latter work.<sup>25</sup>

In retrospect, it has become clear that the logical positivists read Wittgenstein's *Tractatus* against a Machian background which led them to disregard crucial features of it. The book is not an exposition which as a whole could be exemplified by any of the traditional tenets of empiricism; it is rather, as Wittgenstein remarked subsequently, a sort of "Platonic myth," a metaphor in its own right. This separates the early Wittgenstein decisively from the main line of development of logical positivism, even if Schlick and his associates saw themselves as continuing along the path Wittgenstein had opened up.<sup>26</sup> The *Tractatus* influenced the growth of logical positivism particularly with respect to the argument for the distinction between the analytic and the synthetic. There are no synthetic a priori judgments. Systems of logic or mathematics, deductively derived from axioms, are essentially tautological; any other general claim to knowledge is synthetic, which means that it can be counterfactually shown to be false.

### Logical Positivism and Modern Empiricism

The members of the Vienna Circle in its early days saw themselves as the enthusiastic progenitors of a new enlightenment: as Feigl has described it, carrying on "in the spirit of Hume and Comte, but equipped with more fully developed logical tools."<sup>27</sup> In the writings of the logical positivists the differentiation of what is scientific and what is not became convergent with what is meaningful and what is meaningless. What became called the "Verification Principle" went through numerous versions, as the inadequacy of Schlick's original formulation—that the meaning of (synthetic) statements consists in the method of their verification—became very rapidly apparent. In these later versions, "testability" was substituted for "verification." Obviously it would be mistaken to hold that a statement is meaningful only when we have managed to test its validity; otherwise, with improvements in empirical techniques of validation, previously meaningless statements would suddenly become meaningful ones. So the Verification Principle was altered to hold that a statement is meaningful if there is some means of potentially testing, or "confirming," it. But various major difficulties still remained apparent, the most debated being the status of the principle itself. For if it cannot be subjected to the criterion of testability, if it cannot itself be tested, it should seemingly be dismissed as meaningless.

To attempt to get round this difficulty, the Verification Principle was declared to be a procedural rule, not itself a statement. This helped to indicate that what was at issue was, in some part, a problem of the nature of statements; that is, of what constitutes a statement. This can be illustrated by reference to another dilemma in the early formulations of the Verification Principle, concerning the breadth of its application. If taken as a criterion to be applied very generally to all kinds of moral prescriptions or aesthetic judgments, it has the consequence of eliminating these as meaningless, along with metaphysics and theology. But if it concerns only the meaningfulness of "statements," the implication could be drawn that it supplies a criterion of distinguishing statements from other kinds of judgments, commands, etc. The first, more "radical" version of logical positivism gradually became abandoned in favor of the second, more "liberal" one—especially in the hands of Carnap.<sup>28</sup> The view that the "pseudosentences" of metaphysics are meaningless came to be supplanted by the more sophisticated notion that metaphysical doctrines lack cognitive meaning, although they may have emotive meaning. To borrow an expression of Ayer's originally applied in a slightly different context, the metaphysician is treated less like a criminal than like a patient.<sup>29</sup>

The logical positivists initially classified most of the traditional ontological and epistemological dilemmas of philosophy as belonging to metaphysics, and hence as outside the scope of rational discussion. The disputes between phenomenalism, realism, idealism, and so on were dismissed as meaningless, since there is no way that they can be made to submit to any characterization of the Verification Principle. However, they believed that certain issues relevant to these long-established debates could be sustained, and resolved, if they were treated as debates about appropriate philosophical languages. In this way the back door was left ajar for the incorporation of features within the writings of the logical positivists that were denied public admittance at the front. Carnap's earlier work sets out a version of phenomenalism, although he claimed to be discussing only the relevance of "a phenomenalist language" to scientific procedures. His major work in the first part of his intellectual career, *Der logische Aufbau der Welt*, pursues the theme that the aim of philosophy is to express knowledge as a logical structure of basic certainties. Here Carnap advocates a phenomenalist grounding of such certainties. The only sure knowledge is that which is immediately given as sense data; our knowledge of material objects is secondary and derived.<sup>30</sup> Neurath was mainly instrumental in persuading Carnap to abandon this position, the first of several substantial alterations the latter was to introduce into his views over the course of the years. In order to skirt the suggestion that he was again becoming involved in the sorts of epistemological debates that were prohibited, Carnap referred to his shift from phenomenalism to physicalism as a change of "attitude" and not one of "belief," since this would require a theoretical defense of the falsity of the first and the truth of the latter. However, it is clear enough that there was an underlying theoretical

justification of the change which both Neurath and Carnap accepted: that whereas phenomenalism leads to solipsistic paradoxes, physicalism more readily provides for an intersubjective language in which reports of observations are communicated among observers.<sup>31</sup>

Neurath and Carnap developed their physicalist thesis in some part in direct opposition to the tradition of the *Geisteswissenschaften*, which insisted upon the existence of logical and methodological differences between the natural and the social sciences. Everything, Neurath held, occurs in nature, as part of the physical world. Carnap attempted to express this as a thesis about language; that is, to show that all knowledge can be reduced to the propositions of a physicalist language. This applies as much to our knowledge of minds as to that of happenings in nature. All statements in psychology, according to Carnap, whether they are about mental states of one's own or of others, can be translated into a language which refers to physical events in the body of the person or persons concerned. "On these grounds, psychology is a part of the domain of unified science based on physics. By 'physics' we wish to mean, not the system of currently known physical laws, but rather the science characterized by a mode of concept formation which traces every concept back to state-coordinates, that is, to systematic assignments of numbers to space-time points. Understanding 'physics' in this way, we can rephrase our thesis in a particular thesis of physicalism—as follows: psychology is a branch of physics."<sup>32</sup>

The members of the Vienna Circle were already divided quite considerably among themselves prior to their enforced scattering into exile and Schlick's death in 1936. Hahn, Neurath, and Carnap, the so-called "left wing" of the Vienna Circle, were the main figures in the shift away from the dogmatic views of the earlier days, whereas Schlick and Waismann were more inclined to hold fast to their established views. In later times, the core of the movement was continued in the United States and, to a lesser extent, in Britain. "Logical positivism" lost the clear-cut identity that it previously had and devolved into a more general stream of positivistic philosophy, finding ready contacts with, and having a great deal of influence upon, the traditions of empiricism and pragmatism already strongly ingrained in Anglo-Saxon philosophy. Among the members of the Vienna Circle, Carnap, Neurath, Frank, Gödel, and Feigl went to the United States, as did Reichenbach, von Mises, and Hempel from the Berlin group of philosophers who shared much in common with the former group and the Polish logician Tarski, whose ideas influenced both Carnap and Popper (who came to Britain after spending some time in New Zealand). The influence of these authors over the development of certain core areas of analytic philosophy in the English-speaking world has been very considerable indeed, although tempered in Britain particularly by the influence of "ordinary language philosophy" and the later Wittgenstein. I shall be concerned with two principal, and connected, aspects of the influence of the former group of authors: first, with respect to the philosophy of natural science, the dominance of what has been called



the "orthodox" (by Feigl) or the "received" (by Putnam) model of science; and the elaboration, in the light of these views, of the thesis of the unity of science with respect to the logic of the social sciences.

The orthodox model of science derives from the liberalization of the original logical positivist doctrines, especially as led by Carnap; but it also preserves features that stretch back through to Mach's writings. Mach wanted to reduce experience to relations between simple elements. These elements are sensations, not statements about sensations such as appear in scientific theories. Hence Mach failed to recognize the difference between "formal" and "material" modes of speaking. Statements are frequently couched in such a form that they seem to concern experiences, while in fact they are assertions about other statements: these are called "syntactical sentences" by Carnap. Mach's positivistic philosophy was transformed into logical positivism by the treating of Mach's "elements" syntactically, as components not of experience but of a formal language in which experience is described. Mach's elements became "element sentences" or "protocol sentences"—the simplest sentences, not further reducible, in which the formal language is expressed.<sup>33</sup> A protocol sentence, as in legal transcription of protocols, is supposed to be a statement of experience immediately recorded. Carnap regarded the problem of the form of protocol statements as the basic issue in the logic of science, and his attempts to grapple with it provide the key to some of the major changes in his ideas from his early phenomenalist viewpoint onward. The original view of most of the Vienna Circle was that scientific knowledge rests upon a bedrock of indubitable fact, expressed in the immediacy of sensations as specified by Mach. This is the theme of the *Aufbau*. But just as Neurath rejected phenomenism, he never accepted the existence of the bedrock of certainty as ordered by protocol statements. In his famous analogy, knowledge is like a ship that has to be continually rebuilt even while it remains afloat. Carnap was influenced by this and also came to acknowledge that the thesis that scientific theories could in a fairly simple sense be "reduced" to protocol statements had to be revised and made more elaborate.

Carnap was thus led to place a much greater emphasis than in his very early work upon the role of theoretical concepts in the advancement of scientific knowledge, upon the incompleteness of such concepts, and upon their differentiation from the language of observation protocols. Theoretical concepts, one part of the system of scientific knowledge, cannot be directly derived from, or reduced to, the other part, the language of observation. However, the theoretical language and the observation language are connected by "correspondence rules," whereby observations may be interpreted in light of theories, and vice versa. This conception is the core of the orthodox model. A science such as physics is conceived to be a calculus, in which axioms are the fundamental physical laws. The calculus is not directly interpreted but is a "free floating system" in relation to which other theoretical terms are defined. Some of the latter can be interpreted by semantic rules that relate them to a groundwork of observable fact; but interpretation of the theoretical terms is never complete. The theoretical

cohesion of the system is provided by its hypothetico-deductive character, in which theorems can be deduced from the axioms and hence, via the rules of correspondence, particular observations can be "explained." This is some way from the original emphases of logical positivism insofar as the criterion of "testability" only applies in an immediate way at the level of the observation language—although in the final works of his career Carnap still expressed the belief that a means could be found for differentiating cognitively meaningful theoretical terms from meaningless ones.

The precise nature of correspondence rules has proved a controversial matter among positivistically-minded philosophers. The usual general picture of the relation between the observational and theoretical languages is something akin to Braithwaite's analogy: correspondence rules are the "zip" that fastens together theory and observation; the fastener progressively pulls the two elements of a system of knowledge together, as uninterpreted theorems are transformed into observation statements, expanding the empirical content of the theoretical constructs.<sup>34</sup> The allowing of a detachment between theoretical concepts and observation statements, representing the abandonment of the Verification Principle in anything at all close to its original form, has the virtue, Carnap claims, of allowing for the creative scope of scientific innovation and the wide explanatory power that abstract theory can possess.<sup>35</sup> On the other hand, since it has become generally recognized that observation statements are not unchallengeable, the implication might be drawn that the claimed differentiation between the theoretical and the observation language cannot be drawn clearly at all. For, as Feigl says, most positivistically inclined authors today, even those involved in or close to the original Vienna group, recognize that observation-statements cannot be entirely "theory-free."<sup>36</sup>

The dominant account of scientific explanation developed in modern empiricism is given clearest shape in a famous article by Hempel and Oppenheim.<sup>37</sup> It has stimulated a wide-ranging debate and a great deal of literature, in response to which Hempel has modified and elaborated upon his views as first set down. I shall only summarize the main features briefly here; since its possible application to the social sciences and history has provoked as much discussion as its relevance to natural science, it provides an appropriate transition point for moving on to an appraisal of the influence of positivistic philosophy in sociology. The core idea is that the most precise, scientific form of explanation, although not the only one, is "deductive-nomological" (this has also, following Dray, come to be called the "covering-law model" of explanation). Explanation of an event here involves reference to information supplied by two types of statements which are brought together. These are, first, general laws; and, second, statements that specify particular circumstances in which those laws have application. The statement referring to the event or phenomenon to be explained (the "explanandum") is deduced as a necessity from the conjunction of these two.<sup>38</sup> The objective testing of a scientific explanation hence involves empirical confirmation of the statement describing the initial or "boundary" conditions; empirical confirmation of the laws in relation to which the explanandum is deduced; and logical confirma-

tion of the deduction made. According to Hempel, there is a symmetry, or a "structural equality," between explanation and prediction, since the logical form of the two is the same; a prediction consists in deducing a statement about a future rather than a past event. Deductive-nomological explanation is held to be integral to all "empirical sciences"; however, in the social sciences and history it is often less clearly manifest than in natural science. Hempel offers two reasons for this: the universal laws in question are frequently common-sense ones that are taken for granted implicitly rather than formulated as explicit statements; and, partly because of this, not enough is known about the empirical basis of such laws for us to be able to state them with precision. Historians mostly offer what Hempel calls "explanation sketches," in which the relevant laws and boundary conditions are only vaguely hinted at; explanation sketches can be made more complete, and thus in Hempel's words more "scientifically acceptable," through being filled out by empirical testing of the laws and conditions on which they are based.

This theory of explanation in social science is affirmed by Hempel in conscious contradistinction to the tradition of "interpretative understanding" of the *Geisteswissenschaften*—thus echoing one of the persistent themes of logical positivism. *Verstehen*, or what Hempel refers to as "the method of empathic understanding," is admitted as a component in the method of the social sciences only as a mode of suggesting hypotheses. It is not indispensable for social or historical explanation, and many hypotheses arrived at empathically have then to be established in deductive form and tested empirically. Hempel makes it clear that an empiricist criterion of cognitive meaning has to be applied in the same way here as in the natural sciences. Interpretations of "meaning" that are made in sociology and history "consist either in subsuming the phenomena in question under a scientific explanation or explanation sketch; or in an attempt to subsume them under some general idea which is not amenable to any empirical test. In the former case, interpretation clearly is explanation by means of universal hypotheses; in the latter, it amounts to a pseudo-explanation which may have emotive appeal and evoke vivid pictorial associations, but which does not further our theoretical understanding of the phenomena under consideration."<sup>39</sup>

### Positivistic Philosophy and Modern Sociology

Of the members of the Vienna Circle, Neurath wrote most extensively on social issues and made the most sustained attempt to apply logical positivist views to sociology, which he approached from a self-professed Marxist standpoint. While Neurath was a strong supporter of, and a major influence upon, the thesis that the "scientific way of thinking" in philosophy marked the way ahead in the evolution of human thought, he was more inclined than

the other members of the group to emphasize the importance of the social context of particular philosophical traditions in explaining the hold that such traditions may have over their adherents. Neurath was the main figure who kept logical positivism tied to the general interest in the promotion of social progress characteristic of Comte and of Mach. His Marxism, however, was unobtrusive theoretically, except with respect to his advocacy of physicalism; he rejected dialectical logic, the Hegelian legacy in Marx, no less completely than did his colleagues.<sup>40</sup>

For Neurath sociology is regarded as one segment of the division of labor in the totality of unified science; like every other science, it is "free of any world view."<sup>41</sup> He envisages the coming into being of a system of the sciences in which the laws of each particular science, such as sociology, will be connected with the laws of all the other sciences in a uniform logical structure. Laws, Neurath says, are abstract means of passing from observation statements to predictions; the concept of observation is in turn analyzed in terms of physicalism, involving a "social behaviorism." Neurath's behaviorism bears close affinities with operationalism, which of course has in various general respects run parallel to logical positivism as a whole. In deciding whether a term such as "religious ethos" may be legitimately employed in sociology, we have, according to Neurath, to infer the sorts of observation statements it presupposes as concrete modes of behavior. "Let him [the sociologist] not speak of the 'spirit of the age' if it is not completely clear that he means by it certain verbal combinations, forms of worship, modes of architecture, fashions, styles of painting, etc."<sup>42</sup>

Neurath's writings seem to have had little direct influence in sociology as such. The influence of the writings of the logical positivists has been assimilated into sociology in a much more important and pervasive way through a general acceptance of the model of scientific explanation developed in the phase of the devolution of logical positivism into positivistic philosophy. Since this is so diffuse, it would be out of the question to inquire into it in any detail here. I shall therefore indicate some of the connections between positivistic philosophy and positivistic sociology by illustration. Illustrations are easy to find. One aspect of the broad influence which positivistic philosophy has enjoyed within the social sciences, in the English-speaking world at least, is reflected in the replacement of the term "method" by "methodology." The latter has come to mean nothing more than the analysis of procedures of research; it has little explicit relation to the broader process of reflection on the form and concerns of sociology, which is regarded as the proper task of the "philosophy of the social sciences." Methodology is often presumed to involve no particular philosophical commitments; but most of the leading texts offer a few positivistic trimmings to the package. Thus Lazarsfeld and Rosenberg, for example, quote Bridgman and Hempel with approval, accepting the positivist program of effecting the substitution of a precise, formal language of observation for everyday language as the first demand of a scientific sociology.<sup>43</sup>

Rather than attempting to multiply such examples, I shall concentrate upon

indicating the direction of emphasis of three works which have been widely adopted within the mainstream of contemporary sociology. First, Ernest Nagel's *The Structure of Science*, which has served as a stock reference for innumerable sociological texts and discussions; second, Zetterberg's *On Theory and Verification in Sociology*, a representative and influential discussion of the methodology of social science; and third, Hempel's analysis of functionalism, which connects functional explanation to the deductive-nomological model, thereby reestablishing direct contact between "positivism" in its modern form and "positivism" in the tradition of Comte and Durkheim.

Nagel's book is explicitly indebted to Carnap and Frank (as well as to M. R. Cohen).<sup>44</sup> The work follows something of a Comtean outline: the discussion proceeds from mechanics through physics to biology and the social sciences. The account is anchored in terms of an exposition of deductive-nomological explanation, and the differentiation of languages of observation and theory connected by correspondence rules; biology and the social sciences are distinct from the rest of natural science insofar as the former may make use of teleological or functional explanations. Nagel denies that "teleology" is specifically dependent upon the activities of conscious, reasoning agents or that teleological explanation involves a presumption of final causes. The question of the "subjective" or "meaningful" character of human conduct is taken up at some length. "Interpretative understanding," according to Nagel, involves two characteristics: the assumption that one or more particular individuals are, at a certain time, in certain psychological states; and the assumption of a general principle or law stating the ways in which such states are related both to each other and to "overt behavior." Observational evidence is required for both of these, rather than any kind of empathic identification with the actors whose conduct is to be explained: "we can *know* that a man is fleeing from a pursuing crowd that is animated by hatred towards him is in a state of fear, without our having experienced such violent fears and hatred or without imaginatively recreating such emotions in ourselves—just as we can *know* that the temperature of a piece of wire is rising because the velocities of its constituent molecules are increasing, without having to imagine what it is like to be a rapidly moving molecule."<sup>45</sup> Like Hempel, Nagel accepts that empathy may play a part in the derivation of hypotheses; but such hypotheses have then to be tested by "controlled sensory observations."

Most of the generalizations in the social sciences, Nagel says, are statistical uniformities rather than universal laws. This, however, is not because of any specific features of human behavior as such but is primarily because of the relatively youthful stage of development of sociology, which has not yet developed the conceptual and observational precision necessary to determining exactly the limiting conditions of its generalizations; although he has strong reservations about existing functionalist theories in the social sciences, Nagel apparently believes that such precision may be achieved in principle, although there are various factors likely to prevent its full realization in practice. In any case, statistical rather than universal laws are typical of many

areas of natural science. Statistical generalizations are complemented in the social sciences by functional ones, the latter explaining the maintenance of system states through regulative feedback. The advancement of functional explanations in sociology and anthropology is, as in the case of deductive explanations, hindered by the as yet diffuse character of most social-scientific concepts.

In Nagel's view, the fact that human beings can modify their conduct in light of their knowledge, including potentially their knowledge of generalizations made by sociologists, is not a major source of "difficulty" for social science. It is not in fact something which is unique to the social sciences: in natural science also the observation of a phenomenon can alter the character of that phenomenon. The very statement of the latter implies some awareness of the extent to which what is observed is altered by the process of observation; hence the effects produced by the interaction will either be small and can be ignored, or if large can be calculated and corrected for. The logical character of the "interference" is the same in nature and society, although the "mechanisms involved" are different.<sup>46</sup> The possibility of self-fulfilling and self-negating predictions in the social sciences similarly finds direct analogy in natural science. For example, a computer which guides the firing of a gun may be defective such that it just misses the target; however, the oscillations produced by the transmitting of the (erroneous) calculations could cause the gun to in fact hit the target just because it was originally aimed wrongly.

Nagel's work is consciously directed to a spelling out of "liberalized logical positivism"; that of Zetterberg, on the other hand, is more concerned with describing the conduct of research in sociology and the connection between such research and what he calls "theoretical sociology."<sup>47</sup> It is an attempt, Zetterberg says, to complement the insistence of authors such as Lundberg that sociology should match the scientific rigor of the natural sciences, with a fuller appreciation than Lundberg expressed of the basic importance of theory in science. Zetterberg pays due obeisance to the "humanistic content" of the social sciences, but the main emphasis of his argument is upon the continuity between physics, biology, and sociology. Explanation in sociology, if it is to advance beyond lay knowledge or lay beliefs, must assume the same deductive-nomological form which it has in natural science. "Theory" in sociology is often used very broadly as virtually equivalent to "social thought," Zetterberg says; in his usage, however, it means a set of deductively connected laws to which any particular event, within boundary conditions, can be referred. Zetterberg's description of the formalized language which sociology needs if it is to meet the demands of being an empirical science, in which he draws upon Hempel's analysis of cognitive meaning, implies a strict criterion of reducibility of theoretical terms to the terms of the observation language. In an ideal theory it would be possible to reduce the content of all second-order theoretical concepts to a set of "primitive terms," utilizing the procedures of formal logic. The primitive terms of theoretical sociology as a whole refer to observations of the behavior of actors in interaction.<sup>48</sup>

Zetterberg answers affirmatively the age-old question: are there sociological laws parallel to those discovered in the natural sciences? There are many such laws or theoretical propositions that have been turned up by social science; for example, "persons tend to issue prescriptions that maintain the rank they enjoy in their social structure" or "the more favourable evaluations rank-and-file members receive in a group, the more their ideas converge with those of other group members."<sup>49</sup> Two factors influence the specification of such laws in the sociological literature: the conditions of their application are often only vaguely indicated, and it is not made clear what procedures are necessary to confirm or "verify" them. Everyday life abounds with generalizations that people make of their own conduct or of the activities of others: the task of sociology is to test these so as to turn them from lay hypotheses into confirmed findings and laws, discarding those shown to be invalid. "I think sociology should make a more serious effort to incorporate in its theories the best thoughts (theoretical hypotheses) of the human conditions found in Homer, Dante, Shakespeare, Cervantes, Twain and other great writers, who now provide the lion's share of any educated layman's conception of the human drama. In the end, however, the outcome of the theoretical enterprise should be "high informative content, well backed by experience," that is, laws."<sup>50</sup>

Zetterberg's discussion touches only marginally on functionalism and does not elucidate the relevance of what he has to say for significance of functional explanation in sociology. Nagel has treated the question at some length; but here I shall consider the account provided by Hempel, which is concerned with connecting deductive-nomological to functional explanation.<sup>51</sup> According to Hempel, functional analysis is a form of teleological explanation, the latter referring not to the causes of an event but to the ends to which it is directed. Teleological explanation, however, has traditionally been impervious to empirical testing: Hempel quotes the example of entelechy, or "vital force," as a metaphysical principle which in biology has been involved in unacceptable teleological theories. The problem is to strip functional analysis away from any association with such nontestable vitalistic principles.

In biology, Hempel says, functional analysis is concerned with the explanation of a recurrent activity (e.g., the beating of the heart) in terms of its contribution to a state (e.g., the circulation of the blood through the body) of the organism required for the maintenance of life. In the social sciences, the objects of analysis are similarly patterned and repetitive modes of social conduct examined in relation to states of the larger social system. But what is the explanatory element in functional explanation? It is not to be found in the type of nomology characteristic of either deductive-nomological or inductive statistical explanation. There is a close similarity in logical form nevertheless. When, in giving physical explanations, we say that an ice cube melts because it was put into warm water, we are able to justify this as an explanation of the melting by reference to general laws of which the specific case is an instance. In a similar way, the "because" of functional explanation implies a principle

such that, within specified conditions, a system will either invariably or with a high degree of probability meet the functional exigencies needed for its survival in the face of forces threatening to change it. That is to say, the general propositions involved in functional analysis refer to the self-regulation of biological or social systems; thus understood, they yield predictions which can be objectively tested.<sup>52</sup> This depends upon defining concepts like "system need" operationally. "It will no doubt be one of the most important tasks of functional analysis in psychology and the social sciences to ascertain to what extent such phenomena of self-regulation can be found, and can be represented by corresponding laws. Whatever specific laws might be discovered by research along these lines, the kind of explanation and prediction made possible by them does not differ in its logical character from that of the physical sciences."<sup>53</sup>

The three examples I have chosen here are arbitrary, insofar as they could have been replaced by many others expressing similar views—although each has been influential in its own right. I do not want to claim, of course, that the general standpoint they represent has ever become an unrivaled one, but it has undoubtedly been until quite recently the dominant approach in English-speaking sociology. This is not just because the main tradition has insisted that the social sciences should model their aspirations on the sciences of nature; rather, many authors in the former field have accepted, explicitly or implicitly, that "science" can be identified with the positivistic philosophy of science. Functionalism has played an important part in this, as the conceptual vehicle of the continuity between natural and social science; the division between the physical and the life sciences appears as great, if not larger, than that between biology and sociology.

### The Postpositivistic Philosophy of Science

In the philosophy of science, as contrasted to the methodological self-understanding of the social sciences, the "orthodox model" has long since become subject to broad-ranging attack led by such authors as Toulmin, Feyerabend, Hesse, Kuhn, and others. While these writers disagree about the conclusions that should be drawn from their critical analyses of positivistic philosophy, it is clear that they have successfully displaced the orthodox model: it is an orthodoxy no longer. The work of Karl Popper, however, precedes theirs and in some part is one of their sources; a tracing of the critical views which Popper has expressed of logical positivism, as well as the evident themes which connect his writings to those of the Vienna Circle, necessarily precedes any commentary on the "newer philosophy of science" of the past two decades.

The relation between Popper's views and those of the leading members of the Vienna Circle, particularly Carnap, has been a controversial one from the beginning. Popper was not himself a member of the circle, but he had a close intellectual contact with it. His first and still his major work, *Logik der Forschung*, was discussed within the group and regarded as basically in accord with the perspective of logical positivism. Popper, on the other hand, emphasized that the work was radically critical of the philosophy of logical positivism and since its first publication has continued to stress the differences between his position and any kind of empiricism or positivistic philosophy.<sup>54</sup> The points at issue are not easy to disentangle. In assessing the differences between Popper's ideas and those of logical positivism, even in its more liberalized versions, one should mention the following of Popper's views as the most distinctive sources of contrast: his complete rejection of induction and his concomitant rejection of "sensory certainty," whether manifest as phenomenalism or physicalism; his substitution of falsification for verification, with the corresponding stress upon boldness and ingenuity in the framing of scientific hypotheses; his defense of tradition which, in conjunction with the operation of the critical spirit, is integral to science; and his replacement of the logical positivist ambition of putting an end to metaphysics by revealing it as nonsense with the aim of securing criteria of demarcation between science and pseudoscience. These differences are certainly considerable, and underlie Popper's continual insistence that not only is he not a "positivist," but he is one of its foremost critics in the philosophy of science. However there also some clearly apparent, major similarities between Popper's writings and those of the logical positivists. Popper shares the conviction that scientific knowledge, imperfect though it may be, is the most certain and reliable knowledge to which human beings can aspire; his endeavor to establish clear criteria of demarcation between science and pseudoscience shares much of the same impetus as the concern of the logical positivists to free science from mystifying, empty wordplay; and, like the logical positivists, his characterization of science is a procedural one: science is separated from other forms of tradition insofar as its theories and findings are capable of being exposed to empirical testing and therefore to potential falsification.

Popper's first formulation of the principle of falsification as the key to the demarcation between science and nonscience was arrived at, according to his own testimony, as a result of reflection upon the gulf between certain types of social theory—especially Marxism and psychoanalysis—and the physical sciences. The former, Popper came to the conclusion, had more in common with primitive myths rather than with science; they are more like astrology than astronomy.<sup>55</sup> The reason for this, according to Popper, lies less in their lack of precision, as compared to physics, than in what to their adherents is their most attractive characteristic: the range of their explanatory power. As total systems of thought, they gain their support from a quasi-religious experience of conversion or revelation, and, once converted, the believer is

able to explain any event in terms of them. Since they can explain anything or everything, there is no source or type of empirical evidence that could be pointed to as a basis of showing the ideas involved to be mistaken. This stands in marked contrast to relativity theory in physics, which generated specific predictions about the movement of material entities and delivered itself as a hostage to the outcome of the testing of those predictions; such an element of risk is absent from theories such as Marxism and psychoanalysis, which protect themselves against counterfactual evidence. The distinctive characteristic of science, therefore, is that instead of merely seeking confirmation or verification of a theory, the scientist attempts to refute it. Confirmation, or what Popper has subsequently come to call "corroborating evidence," of a theory results from its successful withstanding of empirical assaults which have the aim of falsifying it. "One can sum up all this by saying that *the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*"<sup>56</sup>

Popper's emphasis upon falsification stands in the closest possible relation to the critique of inductive logic with which he began his *Logik der Forschung*. A major tension had always existed at the heart of empiricist philosophies of science. Science was supposed to yield certain knowledge; on the other hand, the logical form of the induction of laws from observations precludes certainty. However many tests we may make confirming a theoretical proposition, there always remains the possibility that the next test will disconfirm it were it to be made; hence the validity of scientific laws can never be conclusively verified. Popper's response to this classical problem of empiricism is to deny the premise on which it rests; that is to say, he denies that science proceeds through induction at all, and he accepts that no abstract proposition in science can ever be finally verified. There is, as Popper puts it, an asymmetry between verification and falsification. No matter how many white swans we may observe, this does not justify the conclusion "all swans are white"; but while such a universal statement cannot ever be derived from singular statements reporting observations, it can be contradicted or shown to be wrong by singular statements.<sup>57</sup> Thus, although Popper's philosophy of science is skeptical in the sense that it accepts that no scientific law, even one which scientists may feel is completely and securely founded, can be conclusively proved; it insists that scientific advance is possible through the empirical refutation of hypotheses. The object of science is still conceived of in a traditional manner as the securing of abstract generalizations that are true insofar as they correspond to facts; but we can never be logically certain that we have attained truth, although we can approach closer and closer to such certainty by the elimination of false theories.

Just as scientific theories are not tested inductively, neither are they arrived at inductively; the manner in which a theory is discovered or invented has nothing to do with its scientific status, which depends solely upon its being able to specify falsifying conditions and being able to withstand empirical testing of those conditions. There is no "logic of discovery," since new ideas

may be conceived as a flash of intuition or as the result of religious reflection or in many other contexts. Nor is there any "observations" which is prior to "theory" in the manner integral to the notion of inductive logic, and fundamental to logical positivism in the form of protocol statements. All observations are "theory-impregnated," and are interpretations of facts. There can be no foundation of certain or incorrigible knowledge upon which science builds, as logical positivism, and positivistic philosophy more generally, assumes. Scientific knowledge is built on shifting sand, and what is important is not where we begin but how far we are able to subject our conjectures to empirical tests and hence to rational criticism. This also supplies the guiding thread in Popper's social philosophy. An "open society" is one in which no single system of ideas is able to monopolize the social order—where freedom is ensured by the critical confrontation of diverse ideas and policies whose outcomes can thus be rationally assessed.

Popper has consistently attempted to separate his thought from the preoccupation with language characteristic of so much contemporary philosophy, holding that the latter obscures the true nature of the scientific enterprise, which is above all concerned with the relation between hypotheses and the world of real objects and events. Terminology, Popper says, does not matter, save insofar as clarity and unambiguity of expression are demanded for the rigorous testing of scientific theories. The same ideas can be expressed in different words; all that matters is that they should be clearly expressed and formulated in such a way that the circumstances in which they can be declared to be falsified are known. Popper's philosophy possesses the boldness of formulation that he requires of science itself: the appeal of his substitution of falsification for verification derives in large part from the simple and incisive way in which it disposes at a stroke of the traditional dilemmas of induction. But the simplicity of the notion is belied by difficulties which it conceals, consideration of which forces us to confront more directly issues of language which Popper tends to dismiss as being at most of only marginal importance.

In the first place, the notion of falsification sits uneasily in Popper's writings with his commitment to a correspondence theory of truth. The aim of science, according to Popper, is more accurately described as concerned with "verisimilitude" rather than truth.<sup>58</sup> But the idea of verisimilitude is only defensible if we assume that there are a finite number of possible conjectures or theories about nature such that by progressively refuting them we come closer and closer to the truth. There seems no warrant for such an assumption, all the less so given Popper's injunction that it is incumbent on the scientist to look for "unlikely" hypotheses since these are the easiest to test. Second, the very idea of falsification, which looks so concise and clear presented as a logical solution to difficulties of induction, becomes quite murky when applied to the analysis of actual scientific activities of testing and the comparison of theories. Popper, of course, acknowledges that the logic of falsification is in some part separable from its implementation in scientific procedures. The universal statement "all swans are white" is in principle

contradicted by the discovery of a black swan, but in practice matters are not so simple, because we have to decide, for one thing, what is to count as a black swan—that is, as a falsifying observation. It would be possible, for example, for someone accepting the universal statement "all swans are white" to discount any case of a black swan that might be found as not being a swan at all, hence placing it outside the scope of the law. Popper's response to such a tactic is to declare it unscientific, alien to the spirit in which science should be carried on. But this is not very convincing, and one could claim that here Popper is hoist with his own petard, because such an argument seems to do just what it criticizes: namely, to propose that any instance which does not accord with the thesis should be disregarded as "unscientific procedure." One of the consequences of Kuhn's work is to affirm that this will not do, and the same holds for that of Feyerabend and Lakatos—in spite of the fact that the latter author regards Popper as the main originator of what he calls "sophisticated falsificationism."

Kuhn's most important study, *The Structure of Scientific Revolutions*, has become very well known indeed, and there is no need to do more than refer in the most cursory manner here to its main themes. Kuhn's views may differ considerably in certain respects from those of Popper, but they also connect up closely with them, because both authors recognize the significance of the history of science for the philosophy of science (and vice versa). This has not been true, by and large, of the logical positivists, who have concentrated primarily upon producing abstract, formal analyses without giving any detailed attention to the historical study of the development of science. Hence, as Kuhn points out, they have tended to operate with accounts of scientific discoveries as finished achievements, as they are recorded in textbooks; but these no more satisfactorily describe the substance of what actually happens in science than tourist brochures describe the culture into which they initiate the traveler.<sup>59</sup>

Kuhn's work was partly stimulated by his awareness of a contrast between the natural and social sciences, not of the kind traditionally stressed in the *Geisteswissenschaften*, but concerning the lack of agreement among social scientists over the basic character of their intellectual endeavors. The social sciences, in short, lack "paradigms." Thus they do not show the characteristic pattern of development of the natural sciences, which is one of periods of relatively stable "normal science," involving puzzle-solving activities within the confines of a shared paradigm, interspersed with periods of revolutionary change as a result of which a new paradigm comes to supersede the old. Revolutions are written *out* of textbooks of science, or rather, never written *in* them; a textbook expresses a paradigm as the consolidated achievements of a particular science to date. Periods of revolutionary change in science are nonetheless a consequence of the activities of normal science, for it is through the puzzle-solving activities of normal science that contradictions or anomalies emerge within the existing framework of knowledge. A revolution in science is a change in world view, a gestalt switch; the conceptual transformation thus effected infuses "observation" itself. "Is sensory experience fixed



and neutral? The epistemological viewpoint that has most often guided Western philosophy for three centuries dictates an immediate and unequivocal, Yes! In the absence of a developed alternative, I find it impossible to relinquish entirely that viewpoint. Yet it no longer functions effectively, and the attempts to make it do so through the introduction of a neutral language of observations now seem to me hopeless."<sup>60</sup>

*The Structure of Scientific Revolutions* has provoked a great deal of discussion, to which Popper, among many others, has contributed. In the course of this debate, Kuhn has attempted to clear up ambiguities in the original work and to elaborate upon it in various ways. I shall concentrate only upon issues relevant to the subsequent sections of this study. The most useful way to identify these is to indicate some of the differences of emphasis in Kuhn's work as compared to that of Popper. Three such differences are the following: (1) For Kuhn, "normal science" is integral to scientific progress, since the suspension of criticism involved in the common acceptance of a paradigm makes possible a concentration of effort upon clearly defined problems. Constant critical assessment of the most basic elements of a "disciplinary matrix" would prevent such a concentration of effort: this is just what occurs in preparadigmatic disciplines, such as the social sciences, in which the inability to agree over basic premises of the substance and method of inquiry blocks the development of knowledge in the form achieved in many areas of natural science. The sort of "permanent revolution" in science envisaged by Popper neither describes the actual conduct of science nor is a desirable framework for it; normal science is not merely deformed science. This view also separates Kuhn from Feyerabend's "scientific anarchism": a proliferation of basic theories is only to be striven for in times of revolutionary crisis. (2) Kuhn's writings demonstrate the hazards in transferring the idea of falsification to the actual practice of science. He says he takes the notion of "the asymmetry of falsification and confirmation very seriously indeed";<sup>61</sup> but "testing" has to be related to the conjunctions of normal and revolutionary science. Scientists working within a paradigm often either ignore or treat as consistent with their accepted theories findings that are subsequently—following the dissolution of the paradigm—recognized as incompatible with or as refuting those theories. (3) Meaning variance, or the "incommensurability" of paradigms, appears as a fundamental problem in Kuhn's work in a way in which it does not in that of Popper; partly as a consequence of this, Kuhn finds Popper's account of verisimilitude unacceptable. Kuhn has consistently denied that he is a relativist, and it is quite obvious that he could not be one: for if the succession of paradigms is not regarded as "progressive," in some sense, the differentiation between preparadigmatic and postparadigmatic sciences effectively loses its significance: on the logical level, successive paradigms would only be "laterally" distributed, each equivalent to any other—the same situation that is claimed to exist in the social sciences. On the other hand, Kuhn has found some considerable difficulty in spelling out how scientific progress occurs through revolutions and what the consequences of the resolution of this problem are for a theory of truth.<sup>62</sup>

## The Critique of Positivism in Frankfurt Philosophy

Since Hume, positivistic philosophers have generally adopted the stance that the sensory experience which provides the basis of scientific knowledge cannot be extended to encompass moral judgments or ethical values. Disputes concerning morality cannot be settled by appeal to intersubjectively available observations as debates over factual issues can. In the social sciences, this has long been the common assumption of most otherwise divergent schools of thought, including various forms of revisionist Marxism (such as that led by Eduard Bernstein). Perhaps the most well-known and influential exposition of the standpoint in sociology is that of Max Weber, who more than any other major writer pursued the implications of the "fact-value dichotomy" to its furthest limits and was prepared to accept these implications in full. For Weber, who drew his views on this issue from neo-Kantianism rather than from British empiricism, the findings of natural or social science stand in a purely instrumental connection to moral values. Science can show us which of a given choice of means is the most effective way of achieving a certain end and what the other consequences of achieving that end are likely to be; but it cannot give us the slightest degree of help in deciding to opt for that end itself (save insofar as that end might be in some part a means to other ends).<sup>63</sup> One consequence of this is that there can be no rational arbitration between the sets of "ultimate values" upon which the major world civilizations rest and which Weber set out to analyze in his studies of the "world religions"; such a clash of values is settled in the area of power struggles.<sup>64</sup>

The imposition of strict limits upon moral reason in positivistic philosophies is something which two generations of Frankfurt philosophers, from Horkheimer, Adorno, and Marcuse to Habermas, have been concerned to criticize. The critique of positivism in this respect has been one of the most central preoccupations of what has come to be called "critical theory." If there is a single dominating element in critical theory, it is the defense of Reason (*Vernunft*) understood in the sense of Hegel and classical German philosophy: as the critical faculty which reconciles knowledge with the transformation of the world so as to further human fulfillment and freedom.<sup>65</sup> Frankfurt philosophy attempts to follow Marx, and thereby to refurbish modern Marxism itself, by appealing to Hegel's transcendence of Kantian dualisms: not only that of pure and practical Reason, but that of the apperception of phenomena and the unknowable "things in themselves." Such dualisms are regarded as both the expression and source of a passive, contemplative attitude to knowledge, an attitude which reduces the practical import of knowledge to "technology" or "technique" robbed of the unifying potentialities of historical Reason. Whereas in Hegel, as Horkheimer puts it, Reason is seen to be inherent in reality, in Hume and in Kant, as well as in Cartesian philosophy, it becomes a "subjective faculty of the mind."<sup>66</sup> The individual subject is the sole possessor of reason, and the latter concept is taken to mean merely the calculative relating of means to ends.

The origins of the "Frankfurt School" were contemporaneous with those of the Vienna Circle, and the members of the former group sharpened their critical assessment of the influence of empiricism in the past by means of onslaughts upon its most prominent representatives in the present. In one such discussion, written in the late 1930s, Horkheimer connects logical positivism to the tradition of Hume and Locke but argues that the critical character which the writings of these authors possessed has been sacrificed by the modern logical positivists.<sup>67</sup> The skeptical empiricism of Hume was subversively directed against the prevailing dogmas in order to forge a new beginning in which rationalism would prevail over the forces of unenlightened mythology. In this sense, the Enlightenment had a moral impetus which in actuality cut across the belief of Hume that facts could be separated from values. This is largely absent from logical positivism, which seeks only to complete and to sanction the domination of science as the contemplative reduction of experience to a logically coherent order of laws. This might be thought unfair to Neurath in particular and untrue to the Marxist leanings of various members of the Vienna Circle. But for Horkheimer such a consideration would be largely beside the point, because Marxism has not stood apart from the positivistic nature of much modern philosophy. On the contrary, the relapse of Marxism into positivistic philosophy is the origin of the twin characteristics of Marxism in the twentieth century: its quietism when in opposition (as in Germany) and its transformation into bureaucratic domination when in power (as in the Soviet Union).

The Frankfurt philosophers have attempted to diagnose the beginnings of "positivistic Marxism" in the writings of Marx himself. What for Althusser and his followers is an "epistemological break" separating the speculative, idealistic Marx from the first formation of scientific Marxism, for the critical theorists marks the phase of the incipient degeneration of Marxism into positivistic philosophy. The Frankfurt authors have differed among themselves about their evaluations of the nature and origins of positivistic Marxism, but their analyses—including that of Habermas in the "younger generation"—have major overall points of agreement. The critical inspiration of Marxism derives from the dialectic of subject and object and is lost where "materialism" means the denial of the active intervention of the subject in history, or the reduction of culture and cultural ideals to epiphenomena of physical events. Monistic materialism, which regards all change as the interplay of natural occurrences, converges directly with non-Marxist positivistic philosophy. Several of the critical theorists have had doubts about the use of the notion of labor in Marx's writings; insofar as this refers merely to the material transformation of nature, and the critique of contemporary society is tied to this, socialism comes to be conceived of merely as a technically more efficient version of capitalism. According to Habermas, in "turning Hegel back on his feet," Marx compressed two elements of Hegel's philosophy into one: man's reflexive awareness as the maker of history and the self-constitution of humanity through labor. When the former is reduced to the latter, the integral tie between history and freedom is dissolved.<sup>68</sup>

In critical theory, "positivism" has a much broader and more diffuse meaning than it does for most other writers, wider even than what I have distinguished as "positivistic philosophy." This use of the term has to be understood against the background of the attempts of the Frankfurt philosophers to effect an ambitious critique of the tendency of development of Western culture since the Enlightenment and, indeed, in certain basic respects since classical times. The progenitors of the Enlightenment set out to effect the disenchantment of the world, to replace myth by solidly founded knowledge, applying that knowledge practically in technology. In so doing they prepared the way for the domination of modern culture by technical rationality—the undermining of Reason against which Hegel struggled and which, with the disintegration of the Hegelian system, became largely lost to philosophy. In the name of freedom from the domination of myth, the Enlightenment created a new form of domination, hidden from view by its own philosophy: domination by instrumental rationality. "Subject and object are both rendered ineffectual. The abstract self, which justifies record-making and systematisation, has nothing set over against it but the abstract material which possesses no other quality than to be a substrate of such possession. The equation of spirit and world arises eventually, but only with a mutual restriction of both sides. The reduction of thought to a mathematical apparatus conceals the sanction of the world as its own yardstick. What appears to be the triumph of subjective rationality, the subjection of all reality to logical formalism, is paid for by the obedient subjection of reason to what is directly given. What is abandoned is the whole claim and approach of knowledge: to comprehend the given as such; not merely to determine the abstract spatio-temporal relations of the facts which allow them just to be grasped, but on the contrary to conceive them as the superficies, as mediated conceptual moments which come to fulfillment only in the development of their social, historical, and human significance."<sup>69</sup>

Critical theory is a defense of just those traditions of philosophy which the logical positivists wished to show consist largely of empty metaphysics. Consequently, it is not surprising that the two schools have kept at arm's distance from one another, and their mutual influence has been slight indeed. However, in recent times, with the increasing strains to which the positivistic philosophy of science has been subject, the influence of the philosophy of the later Wittgenstein and Austin's "ordinary-language philosophy" in Britain and the United States, and of hermeneutic phenomenology on the Continent, the situation in philosophy (as in social theory) has become much more fluid. Among the younger Frankfurt philosophers, Habermas has been particularly influential in connecting critical theory to each of the aforementioned types of philosophy, as well as to pragmatism—while sustaining most of its established themes. Habermas, together with Adorno, played the central part in the controversy over Popper's views that has come to be called (following the usage of critical theory rather than that of Popper) the "positivism debate" in German sociology. The debate is an odd one, insofar as none of the participants regard themselves as defending positivistic philosophy, much less

describe themselves as positivists; given the standpoint of critical theory, however, in which the term "positivism" is applied very broadly to traditions of thought that would not ordinarily be thus designated, it is not difficult to appreciate that the contested meaning of the term is at the heart of the matters at issue, not merely a linguistic curiosity. The initial origin of the dispute was Popper's presentation of "twenty-seven theses" on the logic of the social sciences, at the meeting of the German Sociological Association at Tübingen in 1961; this was followed by a paper by Adorno. Popper and Adorno did not attack each other's contributions directly, however, and their confrontation only ramified into a wide-ranging debate through the subsequent interventions of Habermas, Albert, and others.<sup>70</sup>

In his paper, Popper reiterates his well-known view that the aim of the social sciences is the explanation of conduct through the "situational logic" of action; that is to say, through the rational reconstruction of the circumstances (goals and knowledge) under which individuals act, and of the consequences of their behavior. This is an "interpretative sociology," but not one, according to Popper, that retains any residue of the subjective, empathic qualities with which it has characteristically been associated. It is a "purely objective method."<sup>71</sup> As such, it differs in content but not in logical form from the methods of the natural sciences, which Popper elucidates in terms made familiar by the general corpus of his writings. He rejects what he calls "naturalism" in the social sciences on the same basis as he rejects "positivism" in natural science: naturalism supposes that sociology begins by collecting observations and measurements, inducing generalizations from these data which then become incorporated within theories. This derives from a mistaken (positivistic) philosophy of natural science: the "objectivity" of science lies in its critical method of trial and error. Popper thus affirms his support of "critical rationalism," meaning by this his advocacy of falsificationism as the most integral procedure of science.

Habermas's critique of Popper concentrates mainly upon the limits of Popper's critical rationalism which, according to the former, still contains a strong residue of positivistic philosophy. Popper's theory of science is an analytical, as opposed to a dialectical, one. Habermas suggests that the "objectivity" of natural science cannot be transferred directly to the social sciences, since the latter are concerned with a preinterpreted universe of occurrences; that is to say, with a social world in which the categories of experience are already formed by and in the "meaningful conduct" of human subjects. Hermeneutic understanding, involving the sustaining of communication between the social scientist and those whose conduct he studies, is an essential element of procedure in the social sciences and cannot be encompassed by simple appeal to the "observation" of events in nature, even if transposed as "situational logic." To conceive of the aim of sociology as that of discovering laws has the practical implication of making of it a social technology. "In contrast, dialectical theory of society must indicate the gaping discrepancy between practical questions and the accomplishment of technical tasks—not to mention the realization of a meaning which, far

beyond the domination of nature achieved by manipulation of a reified relation, no matter how skilful that may be—would relate to the structure of a social life-context as a whole and would, in fact, demand its emancipation."<sup>72</sup> To accomplish this, a dialectical or critical theory must transcend the boundaries of critical rationalism as expressed by Popper.

The separation between fact and value, or cognition and evaluation, made in positivistic philosophies, Habermas says, condemns practical questions to irrationality, or to the "closed world" of myth which it is supposedly the object of positivism to dispel. Unlike most philosophers, Popper openly acknowledges this by declaring that his adherence to rationalism is an article of faith. This makes the adoption of rationalism an arbitrary initial decision. Some followers of Popper, notably Bartley, have accepted that there cannot be a deductive foundation for rationalism but have tried to ground critical rationalism by reference to itself; that is to say, by holding that the commitment to critical method as formulated by Popper can itself in principle be criticized.<sup>73</sup> But this will hardly do: Bartley is unable to specify the conditions under which the commitment to rationality would have rationally to be abandoned; this is because what is understood as "criticism" here is too narrow and is not grounded in the historical conditions of human social life and communication. Habermas points to the connection between Popper's adherence to a correspondence theory of truth and the thesis of the dualism of fact and value. Popper shields himself against some of the problems which the correspondence theory raises, when combined with his acceptance of the theory-impregnated character of observation statements, by stressing the difference between knowing what truth means and having a criterion for deciding the truth or falsity of a statement. According to the notion of falsification, we cannot have such a criterion or standard of truth; all we can achieve is the progressive elimination of false views. However, what this involves, Habermas says, is the surreptitious incorporation of standards of evaluation that are uncritically taken over from everyday life; the hermeneutic understanding of ordinary language and intersubjective experience is taken for granted. Critical discussion, as formulated by Habermas, involves three uses of language: the description of a state of affairs, the postulating of rules of procedure, and the critical justification of the former two.<sup>74</sup> Criticism thus cannot be terminated within the sphere of science itself but must concern itself with the standards or values which structure science as one mode of activity among others. So far as the historical context of modern science is concerned, positivistic acceptance of the dualism of fact and value leads to a failure to appreciate that technical rationality supports a system of domination as its legitimating ideology.

Neither Albert, defending Popper, nor Popper himself, in his commentary on the debate, accept that their views do place the sort of bounds upon critical rationalism that Habermas claims. According to Albert, the empirical sciences are able to deal with the type of experience Habermas allots to hermeneutics and can represent these as "facts" like any others. This is, for Albert, potentially a more profoundly critical standpoint than that of Habermas,

since it is a more skeptical one, which finds its critical impetus in the premise that science often shows that assumptions made within the ordinary, day-to-day world are erroneous. Popper's theory of science as myth that is self-critical is the only way of avoiding the twin dilemmas of an infinite regress on the one hand, and the supplying of "foundations" through sheer dogma on the other.<sup>76</sup> Popper's critical rationalism, he repeats, is quite distinct from positivism in all major respects; the critical theorists use the term in such a lax way that they are able to blanket out these differences and hence obliquely charge Popper with some of the very same weaknesses that he has in fact shown to be characteristic of positivistic philosophy. In his comments, Popper concurs: "the fact is that throughout my life I have combated positivist epistemology, under the name of 'positivism.' I do not deny, of course, the possibility of stretching the term 'positivist' until it covers anybody who takes any interest in natural science, so that it can be applied even to opponents of positivism, such as myself. I only contend that such a procedure is neither honest nor apt to clarify matters."<sup>76</sup>

### Comments on the Philosophy of Natural Science

It would obviously be completely out of the question to attempt in this essay a comprehensive discussion of many of the issues raised by the matters referred to in the previous sections. Hence I shall confine my comments to a few problems in two major categories: the philosophy of natural science and the relation between the natural and social sciences.

So far as the first of these is concerned, there are two issues raised by the postpositivistic philosophy of science that loom particularly large. One is the status of falsificationism, as elaborated by Popper and his disciples (particularly Lakatos), and more generally that of deductivist accounts of scientific knowledge, including within this the "deductive-nomological model"; the other is the problem of the "incommensurability" of paradigms such as derived from the writings of Kuhn.

Popper's "solution to the problem of induction," which he has relentlessly advocated from his earliest works, gains much of its attractiveness from its simplicity: the idea that it takes only a single disconfirming instance to falsify a universal statement. But the logic of falsification, he has to admit, is discrepant from the practice. Lakatos's studies, although nominally directed at supporting main elements of the Popperian standpoint, show how wide the discrepancy is. Lakatos distinguishes three kinds of falsificationism: dogmatic falsificationism and naive and sophisticated "methodological falsificationism." The first is the weakest, treating the logical form of falsification as equivalent to its practice: as if a simple observed event or unequivocally defined finite set of events provide the means of refuting scientific theories.

This is an empiricist version of falsificationism, in contrast to methodological falsificationism, which accepts the theory-impregnated character of observations. All testing of theories depends upon acceptance of a theoretical framework which, in any given context, represents unproblematic background knowledge.<sup>77</sup> Naive methodological falsificationism, however, still maintains the view that theories can be refuted, and therefore should be abandoned, in the light of "observations" thus conceived. This will not do because a defender of a theory can always, if he is prepared to be ingenuous enough, "rescue" it from any number of apparently contravening instances. Sophisticated methodological falsificationism recognizes this, and states that there is no falsification where the discarded theory is not replaced by a superior one, where superiority is indexed by the following factors: the second theory has surplus empirical content over the first, predicting facts excluded by or improbable in light of the theory it replaces; the second theory explains all that was explained successfully by the first; and some surplus content of the second theory is corroborated (in Popper's sense of that term). If these criteria are met in any given circumstance of the abandonment of one theory for another, we may speak of a "progressive problem shift." If they are not met, the problem shift is a "degenerating" one; it does not in effect constitute the falsification of the preexisting theory by the one which supplants it.

Lakatos's sophisticated methodological falsificationism is self-confessedly an attempt to reconcile a version of Popper's philosophy of science with some of the major difficulties created for the latter by the works of Kuhn and others. As such, as Kuhn points out, it actually expresses a standpoint quite close to that of Kuhn's own.<sup>78</sup> One of the consequences of Lakatos's emendation of Popper is to downplay the decisionism that brooks large in Popper's own writings (which Habermas emphasizes) and to provide standards for the critical comparison of theories; Lakatos argues that such standards, or "rules of acceptance and falsification," are in fact not provided, or at least are not made explicit, by Kuhn. But the question then arises whether Lakatos, having originally rejected justificationism in favor of fallibilism, has not in the end arrived at a justificationist position which can better be defended and expanded by discarding falsificationism altogether. For Lakatos admits: "'Falsification' in the sense of naive falsificationism (corroborated counter-evidence) is not a *sufficient* condition for eliminating a specific theory: in spite of hundreds of known anomalies we do not regard it as falsified (that is, eliminated) until we have a better one. Nor is 'falsification' in the naive sense *necessary* for falsification in the sophisticated sense: a progressive problem shift does not have to be interspersed with 'refutations.' Science can grow without any 'refutations' leading the way."<sup>79</sup>

As Lakatos uses it, "falsification" (1) only applies to the "degenerating phase" of research programs (in other cases anomalies are largely ignored or accommodated to the existing theory) and (2) only is effective when a better theory supersedes the existing one. It is clear that here refutation no longer forms the main substance of falsification. Lakatos has to all intents and purposes accepted the two major flaws in falsificationism, where that term is

used in a sense that still retains any connection with Popper's critique of inductive logic. The two objections to falsificationism are these. First, in deciding among theories, scientists do not do what Popper's account suggests; that is to say, look for the most bizarre, "unlikely" theory on the grounds that it is the most easily falsifiable. Nor could there be any defense of the thesis that they should do so. Popper's usage here seems to trade on two different senses of what is "unlikely." A theory may be "unlikely" insofar as it is highly innovative; or it may be "unlikely" in the sense that it appears very improbable in light of what is currently regarded as the relevant empirical evidence. Scientists would be wasting their time if they deliberately sought out as often as they could the latter type of unlikely hypotheses. The fact that they do not, however, indicates they operate with an implicit notion of inductive inference.<sup>80</sup> Second, as I have mentioned earlier, Popper's attempt to provide a plausible analysis of scientific progress in terms of "verisimilitude" is unsuccessful, since there is no reason to suppose, within Popper's epistemology, that there is a finite number of potential theories available to interpret any specific range of occurrences.

In rejecting falsificationism, we at the same time reject the Popperian criterion of the demarcation between science and nonscience and the rigid dislocation between the psychology of discovery and the logic of testing. But how can we do so without reverting to the ideas that Popper set out to criticize: those involved in positivistic philosophies of science? In attempting to provide the beginnings of an answer to this question, it is helpful to reconsider the problems that came to light with early formulations of the Verification Principle and subsequently with the liberalized version of logical positivism. The early formulations were based upon the thesis, which stands in direct line of descent from Hume and Mach, that the meaning of scientific concept can in principle always be reduced to empirical observations. The later differentiation between observation and theoretical statements abandoned this standpoint, replacing it with the notion of correspondence rules linking observations and theories; the liberalized model retains the same image of science as a hierarchy of statements built upon a secure foundation of observations. Some of the difficulties created by the distinction between observational and theoretical terms can, as Shapere has pointed out, be linked to this context in which the distinction was elaborated.<sup>81</sup> One such difficulty is that of the ontological status of "theoretical entities." What was no problem in the earlier phase of logical positivism emerges as a major obscurity in its liberalized version. A phenomenalist or physicalist standpoint connects observation terms unproblematically to entities that exist; but it is not clear in what sense a theoretical entity such as an "electron" exists or is some sort of handy fiction. The "surplus content" of a theoretical term—i.e., that which cannot be directly expressed in the observation language—is supposed to be created by the place of the term in the deductive hierarchy of statements. This seems to lead to the uncomfortable and unsatisfactory conclusion that, as there is a continuum from the observable to the unobservable, so there is from objects that exist to ones which do not exist.<sup>82</sup> A second related difficulty concerns the

character of the deductive relations presumed to hold between the levels in the hierarchy of observational and theoretical statements as interpreted axiomatic systems. The "correspondence rules" that intervene between observation and theory are conceived of in a manner parallel to the interpretation of formal systems of mathematical logic, as rules of logical derivation. But logical connections of this sort are obviously different from the connections that may pertain between entities, such as causal relations; and hence we are again led to conclude that theoretical terms are linked to observational ones in such a way that the former do not refer directly to the properties of existent things.

The outline of an alternative scheme, involving a revised model of inductive inference, is suggested by the writings of Quine and has been elaborated in some detail by Hesse.<sup>83</sup> This draws upon Duhem's notion that scientific knowledge should be represented as a network of statements, while not accepting some of the aspects of Duhem's conventionalism.<sup>84</sup> Within this network, what is "observable" and what is "theoretical" can only be distinguished in a pragmatic and relative way. The connecting statements in the network are laws, but laws are treated as pertaining to finite domains; hence one of the classical dilemmas of inductivism, that one cannot move from particular statements to universal ones, is superseded, for all inductive inference involves movement from particulars to analogous particulars. Such a view of scientific laws, Hesse argues, does not imply that universal laws are statistical generalizations or that statistical generalizations are to be regarded as preferable to universal laws in finite domains.<sup>85</sup> Nor does it imply an instrumentalist account of science; rather it implies a realist one, in which the analogical character of theoretical innovation is made central. "Scientific language," as Hesse puts it, "is therefore seen as a dynamic system which constantly grows by metaphorical extension of natural language, and which also changes with changing theory and with reinterpretation of some of the concepts of the natural language itself."<sup>86</sup>

This view of scientific theory does away with the idea of correspondence rules. The network involves observational predicates, which are the "knots" that attach it to the object-world, but these are not a fixed and invariable foundation; where the knots are depends upon the state of development of the theory and the form of its language, and they may be altered in the course of its transformation, especially where the latter is of the "revolutionary" character described by Kuhn. Scientific theory does not involve two languages, a language of observation and a language of theoretical terms; rather, it involves two overlapping and intersecting uses of the same language. Nor is there an absolute differentiation between formal languages of science and natural languages, since the former proceed by metaphorical extension of the latter and of experiences originally organized by the latter in the "natural attitude." In everyday life—and in learning scientific theories—we manage to get to understand observational terms and use them in their relevant contexts, but only by at the same time coming to grasp more abstract terms to which their meanings are connected. If the mode in which this is accom-

plished conforms to the process suggested by Quine, then all descriptive predicates, however "theoretical," are learned in conjunction with definite stimulus situations, or through sentences that contain such predicates (or the two combined). No such predicates, however, are learned by empirical association alone; they do not form an "independent" class of observational terms such as is presupposed in positivistic philosophy. What counts as an observational term cannot be specified without presupposing a framework of accepted laws, which constitute the integrative elements of the network but which in principle and in practice can be radically changed. It is not possible to know, at any given point of time, which laws and predicates may have to be revised or discarded in light of research findings.

The network model of science provides a way of recognizing the poetics of theoretical innovation while at the same time offering a mode of distinguishing sense and reference with regard to "paradigms." Writings such as those of Kuhn, which show the importance of discontinuities in the development of science, push to the forefront two sorts of problems, each potentially posing dilemmas of relativism: one concerns how it is possible to make the transition from one paradigm to another, if they are distinct and different "universes of meaning"; the other concerns how it is possible to sustain a notion of truth, given that the succession of paradigms involves transforming what are recognized as "facts" within divergent systems of theory. The first, the so-called problem of "meaning variance," is in some part an outcome of exaggerating the internal unity of paradigms or of "frames of meaning" more generally.<sup>87</sup> If paradigms are treated as closed systems of concepts whose meanings are determined only by their mutual relation within the system, it becomes difficult to see how transference from one paradigm to another is achieved. The mediation of paradigms or frames of meaning should, however, be more aptly regarded as normal in human experience rather than extraordinary; becoming a scientist, for example, involves distancing oneself from common-sense views of the world as part of the process of mastering scientific theories. The capacity to shift between what Schutz calls "multiple realities," involving the control of allegory and metaphor, is a routine feature of everyday human activity, placed in relief insofar as it is consciously organized as a process of learning new frames of meaning, or one of becoming able to move from one paradigm to another within the context of scientific activity. In this view the mediation of radically discrepant paradigms, such as is involved in scientific "revolutions," is not qualitatively different from meaning transformations required in moving between quite closely related theories; the role of learning by analogy and metaphor is central to both.

The relativistic implications of Kuhn's writing with respect to truth have been a core issue in the debate surrounding his work from the first publication of *The Structure of Scientific Revolutions* up to the present time (although Kuhn himself has consistently rejected relativism in this sense). Such implications also emerge in the writings of some recent philosophers not concerned specifically with the philosophy of science—for example, in the

works of Gadamer in hermeneutics and those of Winch in "post-Wittgensteinian philosophy"—and are one focal point in the respective controversies to which these have given rise.<sup>88</sup> The source of the strain toward relativism is easy to trace: it derives from the idealist leanings of these authors. If "paradigms" ("traditions," "language games") are treated as constitutive of an object-world, rather than as modes of representing or relating to an object-world, there are as many "realities" as there are frames of meaning. Kuhn has made it clear that he does not accept such a view, but without elaborating an account of what notion of truth should replace the versions of the correspondence theory of truth (including that of Popper) which he rejects.<sup>89</sup>

Hesse has suggested that the network model of science involves breaking with the time-honored dichotomy between correspondence and coherence theories of truth, borrowing elements from each while also discarding some of their traditional features; and that this position is most appropriately connected to a realist ontology. Acceptance of the theory-impregnated character of observations has seemed to some to foreclose altogether the possibility of doing what scientists usually claim to be doing; that is, comparing different theories in light of the evidence, since what counts as "evidence" is influenced by the theories themselves—the phenomena can always be saved by the interpretation and reinterpretation of observations. But in this view there lurks a strong residue of positivist philosophy; a purely instrumental account of science is the last refuge of the disillusioned positivist. Against such a standpoint we can pose two integral elements of scientific procedure. One is an insistence upon the significance of sanctioned standards of criticism which help to separate science—although not to demarcate it cleanly—from religious cosmologies. Acknowledgment of the importance of science as self-critique has no necessary connection with a falsificationist epistemology. Indeed, separating the one from the other helps to add force to Habermas's analysis of the shortcomings of Popperianism, by making it clear that the "critical tradition" of science presupposes normative standards that cannot be validated as such in terms of the procedures of scientific testing because they are the legitimating framework within which those procedures are organized. The second point is that the mediation of divergent theories, or paradigms, involves the conjunction of referential parameters which, given the normative orientation of science, always provide an "empirical intersection" subject to disputation with respect to truth claims. This follows directly from the network model of science. The mediation of paradigms is a hermeneutic task, in the sense that it involves the capability of moving between frames of meaning; but such a capability cannot be acquired purely on the level of intension, since the terms comprising the network are tied in in a complex (and variable) way to extensional predicates.

Since the correspondence theory of truth has been traditionally bound up with positivistic philosophies, critics of such philosophies have usually presumed that rejection of them necessitates discarding it also. There are several features of established correspondence theories of truth, however, which are the substantial part of the residue of positivistic philosophy and which can be



separated out without disavowing the correspondence notion altogether.<sup>90</sup> One is the assumption that correspondence theory presupposes at least some statements which are founded upon indisputable observations: statements which are not open to revision. This can be traced in large part to the thesis that the meaning of terms employed in a theoretical language can either be expressed directly as empirical observations or must rest upon a foundation of such observations. The view of language which this involves is an impoverished one, and it muddies over the distinction between the relation of concepts within a theoretical network and the relation between statements involving those concepts and the object-world. The former relation can be illuminated, with respect to truth values, by the incorporation of coherence criteria, or "coherence conditions," as these are suggested by the network model. Such coherence criteria cannot be taken for granted here, as in the positivistic scheme, where the connection between concepts is implicitly explained through the operation of correspondence rules. The criteria can be specified as a set of conditions providing for the interrelatedness of concepts within the networks. The interrelatedness of the components of the network only concerns the object-world with regard to its production as a system of classification; as such this interrelatedness pertains to the network as an organizing medium whereby truth as a relation between statements and the object-world is made possible, but it does not provide the substance of that relation itself.

Two further assumptions deriving from the association of correspondence theory with positivistic philosophies are that advocacy of a correspondence theory presupposes the explication of "correspondence" in some more basic philosophical terms, and that such advocacy necessarily involves providing an account of the existence of the object-world itself. The first gets to the nub of the objections that are traditionally raised against correspondence theory, which concern the difficulty of defining what "correspondence" is.<sup>91</sup> The presumption that such objections have to be answered by specifying the nature of correspondence in terms of some other type of relation, however, is bound up with the positivistic view of the character of observation statements, since observation is taken as a more "primitive" relation than correspondence—that is, as one to which the latter can be in some way reduced. If we break with such a view of observation statements, we can also reject this mode of treating the correspondence relation; "correspondence" then becomes the more primitive term and as such is regarded as a necessary element of the extensional character of a knowledge claim.<sup>92</sup>

The assumption that a correspondence theory has to provide a justification of the independent existence of the object-world is similarly connected with the central concerns of positivistic philosophies, because these are directed toward tying the conditions of knowledge to sensory experience, the latter being taken (in phenomenalism) to actually constitute the object-world. Rejection of positivistic philosophy frees us from the obligation to ground a correspondence version of truth in such a justification, or at least indicates

that an account of the concept of truth does not logically entail it. To propose that the network model of science may be conjoined to a realist epistemology is therefore not to claim that the latter is necessarily the only view which could potentially be reconciled with a reworked theory of truth of the sort suggested here. Moreover, this would involve in turn a detailed reworking of preexisting formulations of "realism."

## The Natural and the Social Sciences

The foregoing discussion of the philosophy of natural science does not provide in and of itself an adequate scheme for a treatment of the connections and divergencies between the natural and the social sciences. It rather indicates some elements of an approach to epistemological problems that span whatever differentiations may exist between them. But the formulation of a postpositivistic philosophy of natural science undoubtedly has direct implications for social-scientific method, which has usually been analyzed against a background of positivistic philosophy, explicitly stated or implicitly assumed. This is not only true of that tradition of thought I began by discussing, which links Comte, Durkheim, and modern functionalism; it also applies to the "counter-tradition" associated with the notion of the *Geisteswissenschaften*.

The contrast between *erklären* [explaining] and *verstehen* [understanding], as portrayed by Droysen and Dilthey, is at the heart of the tradition of the *Geisteswissenschaften*. In establishing his version of this contrast, Dilthey opposed his views to those of authors such as Comte and J. S. Mill who emphasized the continuity of the scientific study of nature and society, stressing instead that the subjective, meaningful character of human conduct has no counterpart in nature. The natural sciences develop causal explanations of "outer" events; the human sciences, on the other hand, are concerned with the "inner" understanding of "meaningful conduct." But Dilthey also accepted important elements of the ideas of Comte and Mill, accentuating the need to make the human sciences as precise and empirical as the sciences of nature. The differences between the natural and the social sciences concern not so much the logical form of their investigations and their results, as the content of their objects of investigation and the procedures whereby they may be studied.

Some of the main tensions in Dilthey's writings (and in those of Max Weber) stem from his attempt to combine elements of positivistic philosophy with the idealistic conception of "life philosophy" taken from the earlier development of the *Geisteswissenschaften* tradition. The "understanding" of human action or cultural products is held to be, following Schleiermacher, a process of the reexperiencing or reenactment of others' inner experiences. But

at the same time, this process is not one of mere intuition; it is one which must be made the basis of a scientific history and which consequently forms the centerpiece of the method of the human sciences. Dilthey's term *Erlebnis* [experience], as Gadamer has pointed out, expresses the strain between the positivistic and idealistic strands in his works.<sup>93</sup> Unlike the verb form *erleben*, the word *Erlebnis* only became common in historical works in the 1870s, largely because of Dilthey's use of it. The word is more restricted than the other German term that may also be translated as "experience," *Erfahrung*, and in Dilthey's writings is introduced as the specific focus of the process of interpretative understanding; in understanding the meaning of what another person does, we grasp the content of that person's "experience" of the world. *Erlebnis* constitutes the fundamental content of consciousness, which Dilthey sometimes refers to as "immediate lived experience"; it is prior to any act of reflection. The term thus ties together the influence of empiricism (only that which can be directly experienced is real) and the influence of life philosophy (the meaningful character of human life is given in the inner experience of consciousness).

The critical response to the *Geisteswissenschaften* tradition on the part of the logical positivists or those close to logical positivism has been a consistent one. *Verstehen* cannot supply the sort of evidence necessary to scientific research, since it depends upon some sort of empathic identification with others. The observation language of social science must refer to overt behavior, not to hypothetical states of consciousness. No matter how much one might try to provide a concrete specification of *Erlebnis*, the latter remains inaccessible to the intersubjectively agreed observations upon which all the sciences must depend. The value of *verstehen*, if it has any at all, is as a mode of suggesting hypotheses; but such hypotheses have to be tested against observations of behavior.<sup>94</sup> In this respect, the views of the logical positivists converge closely with behaviorism in the social sciences.

There are three ways in which this critique of *verstehen* can be assessed: one is in terms of assessing what "understanding" is; another is in terms of assessing what "observable behavior" should be taken to mean; a third is in terms of evaluating the significance of "subjective" elements in conduct. In Dilthey's works, particularly in his earlier writings, *verstehen* is represented as a procedure, or the procedure, whereby the human sciences gain access to their subject matter, and as founded upon some sort of empathic process of "reenactment." The notion that *verstehen* is primarily a mode of procuring data is also taken for granted in positivistic critiques. Thus Abel says that *verstehen* is an "operation" that produces "evidence" and goes on to claim that such an intuitional mode of procedure simply begs the question of whether the process of "understanding" that takes place is a valid one.<sup>95</sup> Such an objection has definite force if the notion of *verstehen* is represented as specific research procedure, and as involving some kind of empathic process; indeed, Dilthey did not successfully manage to reconcile subjectivity and objectivity in the manner in which he sought to do, within a framework

strongly influenced by empiricism. But the dismissal of *verstehen* as a mere propaedeutic writes off major elements of the *Geisteswissenschaften* tradition; the preoccupation with the "meaningful" character of human conduct and culture that characterizes that tradition is abandoned in positivistic philosophy, which attempts to reduce this to the content of "empirical observation." Hence it is important to recognize that recent contributions from within the tradition, as revitalized by hermeneutic phenomenology, have reworked the notion of *verstehen* in such a way as to detach it from its dependence upon the idea of the "reenactment" or "reliving" of the experiences of others. Thus, for Gadamer, *verstehen* is to be treated not as a special procedure of investigation appropriate to the study of social conduct, but as the ontological condition of intersubjectivity as such; and not as founded upon an empathic grasp of the experiences of others, but upon the mastery of language as the medium of the meaningful organization of human social life.<sup>96</sup>

To associate the notion of *verstehen* with language as the medium of intersubjectivity offers direct points of connection with the postpositivistic philosophy of science. Recognition of the significance of frames of meaning, and of their mediation, appears both in Gadamer and in Kuhn, although in the writings of the former it is incorporated into a broad exposition of hermeneutics. Insofar as all "understanding" occurs through the appropriation of frames of meaning, it is no longer regarded as a procedure that distinguishes the social from the natural sciences, but as common to both. The question of the relation between the social and natural sciences can then be seen in a new light. Natural science involves the development of frames of meaning, organized as networks, and discontinuities in the progression of scientific theories pose hermeneutic problems similar to those relating to the mediation of frames of meaning in other spheres of activity. But the social sciences are concerned with a preinterpreted world, in which frames of meaning are integral to their "subject matter"—i.e., the intersubjectivity of practical social life. Social science thus involves a "double hermeneutic," linking its theories, as frames of meaning, with those which are already a constituent part of social life.<sup>97</sup> The ramifications of this, of course, are complex and difficult to trace, involving identifying the relations between lay beliefs and ordinary language concepts on the one hand, and the concepts and theories of the social sciences on the other.

Let us move to the problem of what the notion of "observable behavior" should be taken to refer to. It should be clear that what has already been said about the reformulation of the concept of *verstehen* connects with this, insofar as it helps to indicate the residual difficulties in the claim of positivistically minded critics that *verstehen* is no more than a preliminary source of hypotheses that then have to be matched against behavior. Abel explains this as follows: at the onset of a freezing spell of weather, a man sees his neighbor go out to his woodshed, chop some logs, carry them into the home, and light them in his fireplace. He understands what his neighbor is

doing as "lighting a fire to warm himself because he feels chilly." But he cannot know, without further investigation, that this is correct; the neighbor may, for example, have lit the fire as a signal of some sort to someone else. Hence *verstehen* only provides a plausible hypothesis as to what happened.<sup>98</sup> This conclusion, however, begs one type of question by assimilating it to others. It presupposes that the observer already understands the ordinary language terms "freezing spell," "neighbor," "woodshed," etc. Because such understanding is taken for granted, the question of how it is accomplished is not distinguished from the issues of how behavior may be characterized and in what sense, if any, "subjective" elements are relevant to the explanation of human conduct in the social sciences.

The affiliation of positivistic philosophy with behaviorism stems from a common mistrust of features of conduct that are not "observable," where that term means "directly apprehended by the senses." Rejection of phenomenism or physicalism frees us from some of the restraints of this view, which has never managed to come to terms with the difference between "behavior" and "agency"; that is, between involuntary reactions and acts that are "made to happen" by the individual. The notion of agency or action has been much discussed in the recent philosophical literature, in some substantial part as a result of the emphases of Wittgenstein's *Philosophical Investigations*. Some philosophers, particularly those strongly influenced by Wittgenstein, have argued that human conduct can be described on two discrete levels, one being that of "movements," employing something like the language of behaviorism, the other being that of "actions." To speak of "an arm moving up" is to describe a movement; to speak of "raising one's arm" is to redescribe the movement as an action. But this is misleading if it assumes that these are two alternative modes of description that are equally applicable to any specific form of human conduct. They are more appropriately seen as rival, rather than complementary, types of predicate; to refer to action as if it were merely (reactive) behavior is to *misdescribe* it. In the distinction between "movement" and "action" there is still a residue of the view that only "overt behavior" can be directly observed. But there is no warrant for this if the positivistic view be relinquished; we observe "actions" as directly as we do "behavior."<sup>99</sup>

This still leaves unresolved the status of "subjective elements" in action. Abel's example makes it clear that he is referring to the purposes for which an act may be undertaken: the actor in question lights the fire in order to keep himself from feeling chilly. He employs a behavioristic terminology in expressing this and holds that the event of lighting the fire can only be adequately explained when it is made part of a type of deductive-nomological scheme. The explanation takes the following form: low temperature reduces body temperature; heat is produced by making a fire; the "stimulus" (freezing weather) is connected to the "response" (lighting the fire) via the generalization "those feeling cold will seek warmth." This, as it were, formalizes the assimilation of reactive behavior and action. The scheme

recognizes no difference between cases in which what Abel calls the "feeling states" of an individual are connected by some kind of mechanical effect, and those which are within the scope of his agency. Hence the treatment of purposive components of conduct is thin and barren: purpose or intention appears only as a "feeling state" tying stimulus to response. There is no place for a conception of the actor as a reasoning agent, capable of using knowledge in a calculated fashion so as to achieve intended outcomes.

This is one of the major points at which the line of thought running from Comte and Durkheim to modern functionalism, and modern positivistic philosophy as stemming from logical positivism coincide: in the absence of a theory of action. Each involves a deterministic form of social philosophy, although the logical positivists have regarded as suspect the proclivity of the former for "holistic" concepts such as "conscience collective," "representation collective," etc.<sup>100</sup> The writings of Talcott Parsons have played a major part in connecting Durkheim's works to modern functionalism. Parsons has specifically sought to break with some of the main emphases of positivistic philosophy; he has also formulated an "action frame of reference," originally established in order to incorporate an important element of "voluntarism" into social theory.<sup>101</sup> But the voluntaristic features of Parsons's scheme turn out to depend mainly upon the Durkheimian theorem that the collective values which facilitate social solidarity are also "internalized" as motivational components of personality. The attempt to provide a treatment of voluntarism in the context of a theory of institutions becomes reduced to an emphasis that social analysis needs to embody a theory of motivation, rather than providing a framework that relates motives to the rational monitoring of action.

A developed theory of action must deal with the relations between motives, reasons, and purposes, but must also attempt to offer, as functional theorists have always tried to do, an account of institutional organization and change. For if it is the case that functionalism, even in its most sophisticated form in Parsons's writings, has not been able to produce an adequate theory of action, it is also true that those schools of thought which have been most preoccupied with the philosophy of action, including particularly post-Wittgensteinian philosophy and existential phenomenology, have skirted problems of institutional orders and their transformation. I have suggested elsewhere, following Schutz, that the terms "motive," "reason," and "purpose" are misleading as employed in ordinary terminology because they presuppose a conceptual "cutting into" or segmentation of the uninterrupted flow of action; such a cutting into the ongoing course of action is normally made only when an actor is queried about why he acted as he did, when he reflexively categorizes a segment of his action, or when an observer does.<sup>102</sup> Thus it is more appropriate to regard the aforementioned three terms as processual ones; the subjective orientation of action can then be regarded as directed purposively in conjunction with ongoing processes of the motivation and rationalization of action. The latter implies that the socially competent actor routinely monitors

his action by "keeping in touch" theoretically with the content of what he does; or, expressed in an alternative way, that when asked for an explanation of a specified "segment" of his conduct, he is able to provide one. The problem of connecting the subjective orientation of action to institutional structures has always appeared an enormously difficult one, but this is at least in some part because "structure" has usually been conceived of in a fundamental way as a *constraint* upon action. Durkheim explicitly makes this the defining property of social structure, separating "social facts" from "psychological facts"; if others have been less direct, they have accepted much the same notion.<sup>103</sup> But the structural properties of institutions are not just constraints upon action, they are enabling; a central issue facing social theory in this regard is that of developing a reformulation of the key concepts of "structure" and "system" in such a way as to acknowledge the enabling as well as the constraining aspects of institutional forms. In such a conception, the reflexive rationalization of action must be seen as operating through the mobilization of structural properties, and at the same time thereby contributing to their reproduction.<sup>104</sup>

Recognition of the central importance of such an approach to a theory of action involves repudiating the positivistic tendency to regard reflexivity as merely a "nuisance" and also has direct consequences for the question of the status of laws in the social sciences. Nagel's discussion of self-influencing predictions, referred to previously, is typical with respect to the first of these issues, insofar as reflexivity is treated only from the point of view of prediction and insofar as it is assumed that its influence is a "problem" for the social sciences. Even within these terms of reference, however, "self-fulfilling" and "self-negating" prophecies do not have, as he claims, direct analogies in the natural sciences. The point is the manner in which such things happen, not the fact of their happening, in society and in nature. That is to say, in the sphere of society, as contrasted to nature, self-influencing predictions occur because the predictions made come to be taken over and reacted to as part of the behavior of reasoning agents—as an element of the "knowledge" they apply in the reflexive rationalization of their conduct.

Human beings are reasoning agents who apply knowledge of their contexts of action reflexively in their production of action, or interaction. The "predictability" of social life does not merely "happen" but is "made to happen" as an outcome of the consciously applied skills of social actors. But the scope of the reflexive rationalization of action of concrete individuals is bounded, in several ways; each indicates specific matters of concern for social science. One concerns the formalization of the knowledge that is applied in action. In producing a grammatical English utterance, for example, a speaker demonstrates and draws upon knowledge of syntactical and other rules involved in speaking English; but he is not likely to be able to give a formal account of what those rules are, although he does "know" them—i.e., know how to use them. However, the application of such "knowledge" is made within a parameter of influences that are not part of the ongoing rationaliza-

tion of his action. Such influences include not only repressions and unconscious elements of personality, but also external conditions, including the conduct of other actors. A third boundary of the reflexive rationalization of conduct is found in the unintended consequences of action. This connects closely to the second, insofar as the production and reproduction of institutional structures appear as the unintended outcome of the conduct of a multiplicity of actors.

A crucial point to recognize is that the boundaries between these three types of unacknowledged conditions of action are fluid, as is the scope of the rationalization of action in relation to them. We then have a basis for an analysis of the question of the status of "laws" in the social sciences. Zetterberg suggests that there is no shortage of generalizations in social science: the object of social science should be to make the formulation of generalizations more precise and to verify them in light of empirical research. Zetterberg's discussion follows the characteristic lines of positivistic sociology, in holding that such laws will derive from the progressive accumulation of research and should form a deductive hierarchy. Adoption of the network model of natural science involves rejecting his views. We can represent theories in social science, as in natural science, as networks involving laws or abstract generalizations. But in the second of these the network is not in interaction with the object-world it seeks to explain, whereas in the first it is. Generalizations in the social sciences are always in principle unstable in relation to their "subject matter"—i.e., social conduct and its institutional forms—insofar as their acceptance alters the relation between the rationalization of action and its unacknowledged grounds. This is distinct from the "technical" possibilities of intervention in nature offered by laws in the natural sciences. Knowledge of laws in natural science allows human beings to alter the empirical incidence of the circumstances under which they apply; or, if this be desired, to extend their range. But while knowledge of the laws allows for material transformation in such ways, this does not alter the causal connections involved in or underlying them. In the social sciences, on the other hand, the causal connections that are specified or implied in generalizations depend upon particular alignments of the rationalization of action and its unacknowledged conditions, and hence are in principle mutable in light of knowledge of those generalizations.

The degree to which this happens, and its consequences, are of course limited by practical circumstances. But however this may be, the implication is unavoidable that the relation of social science and its subject matter cannot be handled within a differentiation between "pure" and "applied" science. In a longer study, discussion of this would mean taking up in a direct way the character of social science as critique and offering an analysis of the thesis of the dualism of fact and value. For just as the idea of a transcendental language of observation turns out to be mistaken, so also is the idea of "ultimate values," upon which the notion of the fact/value dichotomy depends; what constitutes a factual statement, and what constitutes a judgment of value, is contextually variable.

## NOTES

1. The influence of Saint-Simon over Marx is a matter of some controversy in itself. For a systematic treatment, see Georges Gurwitsch, "La sociologie du jeune Marx," in *La vocation actuelle de la sociologie* (Paris, 1950).
2. Herbert Marcuse, *Reason and Revolution* (London, 1955) p. 341.
3. Auguste Comte, *Cours de philosophie positive*, vol. I (Paris, 1975) pp. 21ff.
4. *Ibid.*, p. 21.
5. *Ibid.*, pp. 28–29.
6. *Ibid.*, vol. 2, p. 139.
7. *Ibid.*, pp. 139–40.
8. See John Stuart Mill, *Auguste Comte and Positivism* (Ann Arbor, 1961) pp. 125ff.
9. Comte, *Cours de philosophie positive*, vol. I, pp. 44ff.
10. See Herbert Spencer, *Reasons for Dissenting from the Philosophy of M. Comte* (Berkeley, 1968). Mill comments on this in *Auguste Comte and Positivism*, pp. 5ff.
11. See Kurt H. Wolff, *Emile Durkheim et al., Essays on Sociology and Philosophy* (New York, 1964).
12. Emile Durkheim and Fauconnet, "Sociologie et sciences sociales," *Revue philosophique*, vol. 55 (1903).
13. Mill, *Auguste Comte and Positivism*, p. 59.
14. Emile Durkheim, *The Elementary Forms of the Religious Life* (New York, 1965) pp. 170ff; Durkheim and M. Mauss, *Primitive Classification* (London, 1963).
15. Emile Durkheim, *The Rules of Sociological Method* (London, 1964) p. 14.
16. *Ibid.*, pp. 48ff.
17. For a full-scale biography of Mach, see John T. Blackmore, *Ernst Mach, His Work, Life and Influence* (Berkeley, 1972).
18. See Jürgen Habermas, *Knowledge and Human Interests* (London, 1972) pp. 74ff.
19. Comte, *Cours de philosophie positive*, vol. 2, pp. 16ff.
20. Ernst Mach, *The Analysis of Sensations* (Chicago, 1914) pp. 37ff.
21. Ernst Mach, *Erkenntnis und Irrtum* (Leipzig, 1917) p. VII.
22. Mach, *The Analysis of Sensations*, p. 369.
23. See *inter alia*, Victor Kraft, *The Vienna Circle* (New York, 1953). Mach's theories also attracted the attention of prominent literary figures. Hofmannstahl, the poet, attended Mach's lectures, believing that if the world consists only of our sensations, it can be described more directly and thoroughly in poetry than in science. Robert Musil began his career as a philosopher, actually writing a doctoral thesis on Mach, before turning to the novel form.
24. A. J. Ayer et al., *The Revolution in Philosophy* (London, 1956).
25. Rudolf Carnap, "Intellectual Autobiography," in Paul Arthur Schilpp, *The Philosophy of Rudolf Carnap* (La Salle, 1963) pp. 12ff.
26. Stephen E. Toulmin, "From Logical Analysis to Conceptual History," in Peter Achinstein and Stephen F. Barker, *The Legacy of Logical Positivism* (Baltimore, 1969) pp. 31ff. Carnap later wrote on this point, "when we were reading Wittgenstein's book in the Circle, I had erroneously believed that his attitude towards metaphysics was similar to ours. I had not paid sufficient attention to the statements in his book about the mystical, because his feelings and thoughts in this area were too divergent from mine." Carnap, "Intellectual Autobiography," p. 27.
27. Herbert Feigl, "The Origin and Spirit of Logical Positivism," in Achinstein and Barker, *The Legacy of Logical Positivism*, p. 5.
28. See Rudolph Carnap's preface to the second edition of *The Logical Structure of the World* (London, 1967).
29. A. J. Ayer, editor's introduction, in *Logical Positivism* (Glencoe, Ill., 1959) p. 8.
30. Carnap, *The Logical Structure of the World*.
31. Carnap, "Intellectual Autobiography," p. 52.
32. Carnap, "Psychology in Physical Language," in Ayer, *Logical Positivism*, p. 197.
33. See Richard von Mises, *Positivism, a Study in Human Understanding* (Cambridge, Mass., 1951) pp. 80ff.
34. Richard Bevan Braithwaite, *Scientific Explanation* (Cambridge, 1968) p. 51.
35. See Carnap, "The Methodological Character of Theoretical Concepts," in Herbert Feigl and Michael Scriven, *The Foundations of Science and the Concepts of Psychoanalysis* (Minneapolis, 1956).

36. Herbert Feigl, "The 'Orthodox' View of Theories: Some Remarks in Defence as well as Critique," in M. Radner and S. Winokur, *Minnesota Studies in the Philosophy of Science*, vol. 4 (Minneapolis, 1970).
37. Carl G. Hempel and P. Oppenheim, "Studies in the Logic of Explanation," *Philosophy of Science*, vol. 15 (1948).
38. Carl G. Hempel, "Deductive-Nomological vs. Statistical Explanation," in Herbert Feigl and Grover Maxwell, *Scientific Explanation, Space, and Time* (Minneapolis, 1962).
39. Carl G. Hempel, "The Function of General Laws in History," in *Aspects of Scientific Explanation* (New York, 1965) pp. 240–41.
40. Carnap, "Intellectual Autobiography," p. 24.
41. Otto Neurath, "Sociology and Physicalism," in Ayer, *Logical Positivism*, p. 283; see also Otto Neurath, *Foundations of the Social Sciences, International Encyclopaedia of Unified Science*, vol. 2 (Chicago, 1944).
42. Neurath, "Sociology and Physicalism," p. 299.
43. Paul F. Lazarsfeld and Morris Rosenberg, "General Introduction," in *The Language of Social Research* (New York, 1955) pp. 2ff.
44. Ernest Nagel, *The Structure of Science* (London, 1961) p. X.
45. *Ibid.*, p. 484.
46. *Ibid.*, pp. 468–69.
47. Hans L. Zetterberg, *On Theory and Verification in Sociology* (Totawa, N.J., 1966).
48. *Ibid.*, pp. 46ff. Compare Hubert M. Bialock, Jr., *Theory Construction* (Prentice-Hall, N.J., 1967) pp. 2ff and 10ff.
49. Zetterberg, *On Theory and Verification in Sociology*, pp. 81 and 85.
50. *Ibid.*, pp. 102–3.
51. Hempel, "The Logic of Functional Analysis," in *Aspects of Scientific Explanation*.
52. *Ibid.*, p. 317.
53. *Ibid.*, p. 325.
54. See Popper's autobiographical article in Paul Arthur Schilpp, *The Philosophy of Karl Popper* (La Salle, 1974).
55. See, e.g., Karl Popper, "Science: Conjectures and Refutations," in *Conjectures and Refutations* (London, 1972) pp. 34–37.
56. *Ibid.*, p. 37.
57. Karl Popper, *The Logic of Scientific Discovery* (London, 1972) pp. 41ff.
58. See, e.g., "Two Faces of Common Sense," in *Objective Knowledge* (Oxford, 1973) p. 57ff. For a critical discussion of Popper's use of Tarski's theory of truth, see Susan Haack, "Is It True What They Say About Tarski?" *Philosophy*, vol. 51 (1976). On "verisimilitude" see David Miller, "Popper's Qualitative Theory of Verisimilitude," *Int'l Journal of the Philosophy of Science*, vol. 25 (1974).
59. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1970) p. 1.
60. *Ibid.*, p. 126.
61. See Thomas S. Kuhn, "Reflections on My Critics," in Imre Lakatos and Alan Musgrave, *Criticism and the Growth of Knowledge* (Cambridge, 1970) p. 248.
62. See Thomas S. Kuhn, "Second Thoughts on Paradigms," in Frederick Suppe, *The Structure of Scientific Theories* (Urbana, 1974). For Popper's most recent reflections on similar issues, see Karl Popper, "The Rationality of Scientific Revolutions," in Rom Harré, *Problems of Scientific Revolution* (Oxford, 1975).
63. Max Weber, *The Methodology of the Social Sciences* (Glencoe, Ill., 1949) pp. 13ff.
64. See Anthony Giddens, *Politics and Sociology in the Thought of Max Weber* (London, 1972).
65. Marcuse, *Reason and Revolution*, pp. 6ff.
66. Max Horkheimer, *Eclipse of Reason* (New York, 1974) p. 5.
67. Max Horkheimer, "Der neueste Angriff auf die Metaphysik," *Zeitschrift für Sozialforschung*, vol. 6 (1937).
68. Jürgen Habermas, *Knowledge and Human Interests* (London, 1972) pp. 43ff.
69. Max Horkheimer and Theodor W. Adorno, *Dialectic of Enlightenment* (New York, 1972).
70. Theodor W. Adorno, et al., *The Positivist Dispute in German Sociology* (London, 1976). First published in German in 1969. A new controversy has now appeared, crossing some of the lines of the first, in which followers of Popper have moved to the offensive in launching an attack on the "politicization of science." For a discussion, see Ralf Dahrendorf, "Die Unabhängigkeit der Wissenschaft," *Die Zeit* (May 21, 1976) and the reply by Lobbkowitz in the same issue.
71. Popper, "The Logic of the Social Sciences," *Positivist Dispute*, p. 102.

72. Jürgen Habermas, "Analytical Theory of Science and Dialectics," *Ibid.*, p. 142.
73. William W. Bartley, *The Retreat to Commitment* (London, 1964).
74. See Habermas, *Knowledge and Human Interests*, pp. 301ff.
75. Hans Albert, "Behind Positivism's Back?" in Adorno, *The Positivist Dispute in German Sociology*, pp. 246ff. See also, Hans Albert, *Traktat über kritische Vernunft* (Tübingen, 1968).
76. Popper, "Reason or Revolution?" *Positivist Dispute*, p. 299.
77. Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes," in Lakatos and Musgrave, *Criticism and the Growth of Knowledge*, pp. 106ff; see Imre Lakatos, "Changes in the Problem of Inductive Logic," in *The Problem of Inductive Logic* (Amsterdam, 1968).
78. Kuhn, "Reflections on My Critics," pp. 256ff.
79. Lakatos, *The Problem of Inductive Logic*, p. 121. See also footnote 4, p. 122; also p. 137, where "verification" is reintroduced, albeit reluctantly.
80. Mary Hesse, "Positivism and the Logic of Scientific Theories," in Achinstein and Barker, *The Legacy of Logical Positivism*, p. 96.
81. Dudley Shapere, "Notes Toward a Post-Positivist Interpretation of Science," *Ibid.*
82. *Ibid.*, p. 127.
83. See, *inter alia*, W. O. Quine, *From a Logical Point of View* (Cambridge, 1953); *Word and Object* (New York, 1960); *Ontological Relativity and Other Essays* (New York, 1969); Mary Hesse, *The Structure of Scientific Inference* (London, 1974).
84. See Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton, 1954); *To Save the Phenomena* (Chicago, 1969).
85. Hesse, *The Structure of Scientific Inference*, pp. 175ff.
86. *Ibid.*, pp. 4-5.
87. See Anthony Giddens, *New Rules of Sociological Method* (London, 1976) pp. 142ff; see also Israel Scheffler, *Science and Subjectivity* (Indianapolis, 1967) pp. 80ff.
88. Karl Otto Apel, *Hermeneutik and Ideologiekritik* (Frankfurt, 1971); Brian Wilson, *Rationality* (Oxford, 1970).
89. See Kuhn, "Second Thoughts on Paradigms."
90. Hesse, *The Structure of Scientific Inference*, pp. 57ff.
91. See the Austin-Strawson debate, in George C. Pitcher, *Truth* (N.J.)
92. Hesse claims that this standpoint is consistent with Tarski's "semantic conception" of truth.
93. Hans-Georg Gadamer, *Truth and Method* (London, 1975) pp. 55ff.
94. Theodore Abel, "The Operation Called *Verstehen*," *American Journal of Sociology*, vol. 54 (1948); Carl Hempel, "On the Method of *Verstehen* as the Sole Method of Philosophy," *The Journal of Philosophy*, vol. 50 (1953).
95. Abel, "The Operation Called *Verstehen*."
96. Gadamer, *Truth and Method*; see also Idem: *Kleine Schriften* (Tübingen, 1967).
97. See Giddens, *New Rules of Sociological Method*, pp. 148ff.
98. Abel, "The Operation Called *Verstehen*."
99. See John G. Gunnell, "Political Inquiry and the Concept of Action: A Phenomenological Analysis," in Maurice Natanson, *Phenomenology and the Social Sciences* (Evanston, 1973).
100. For a definition of "determinism" here, see Giddens, *New Rules of Sociological Method*, p. 85.
101. Talcott *The Structure of Social Action*. (Glencoe, Ill., 1949).
102. Giddens, *New Rules of Sociological Method*; Alfred Schutz, *The Phenomenology of the Social World* (London, 1972).
103. The idea of "structure," of course, appears in many varying contexts in modern thought. There are obvious contrasts between the mode in which the term is used in "structural-functionalism" on the one hand, and "structuralism" on the other. For relevant surveys, see Raymond Boudon, *The Uses of Structuralism* (London, 1971); Jean Piaget, *Structuralism* (New York, 1970); Peter M. Blau, *Approaches to the Study of Social Structure* (New York, 1975).
104. See my analysis offered in Anthony Giddens, "Functionalism: *après la lutte*," *Social Research*, vol. 43 (1976).